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REVIEWS

Memoirs of George Monk, Duke of Albemarle. From the French of M. Guizot. Translated and edited, with additional notes and illustrations, by the Hon. J. Stuart Wortley. Bentley.

SOLOMON has recorded, that there were three things in nature which passed his comprehension: had the present volume been placed before him, he would have added at least two more to the number,—the writing, and the translating of such a piece of biography. To the composition of a history (whether of a man or an event) there are two principal inducements. We may be drawn to a subject, either by the greatness of the action, or by the dignity and importance of the actors. The thing to be known may be valuable to mankind as mere matter of fact, or by the light it throws on the better part of human nature,—as a link in the chain of social developement, or as an example of the force of virtue, of the blessings it diffuses, and of the illustration it confers. In other words, the narrative may address itself to the intellectual, or to the moral man; but if it be applicable to neither of these purposes, the information is a mere burthen to the memory, and the perusal an idle amusement, or a wearisome task.

Influenced by such considerations, we received the announcement of M. Guizot's work with a surprise, to which the character both of the subject and the author equally contributed. That any one should have thought it worth while to illustrate the life of a man, of whom Guizot himself justly observes, that he was "at once celebrated and obscure,"—that he was "one of those whose talents and even vices have but a day or hour for the developement of their full energy and dominion,"—one, who, being renowned for that day, was "on those which either precede or follow it, scarcely to be distinguished from the crowd,"—that any author should count upon giving interest to such a personage, would have been an unexpected chance; but that Mons. Guizot should have been the person, was altogether past expectation. The contributions which this writer has made to historical literature, are too well known to require comment; and the mere mention of his name at once suggests the idea of large and comprehensive views, a luminous philosophy, and transcendental ethical results. History, under his intellectual grasp, exhibits the unity of action and of moral which characterizes the regular epic; and, in his hands, it acquires that gravity and dignity which Aristotle has assigned exclusively to poetry.† Little, therefore, were we prepared to find him wasting his powers on one whose actions were an episode only in the great drama in which he took a part, and were utterly incapable of yielding either an example or an illustration.

It would have been scarcely possible to hit upon any personage having played a conspicuous part in English history, who could afford so little matter of public interest as Monk. That he was the immediate agent in bringing about the restoration, may be a sufficient reason for placing him somewhat conspicuously forward in a general picture of that event, because no such picture would be complete without it. But there is neither in his share of that transaction, nor in the qualities that placed him in the position for effecting what he did, *stuff enough* for a substantive biography. That it was "in the sole power of Monk to accomplish the event," as assumed by M. Guizot, seems to us the very contrary of truth. So far from being the guiding genius of the restoration, he seems to have been the mere sport of accident,—or, at least, one only of those waiters upon Providence, who, in the language of the Stock Exchange, watch the turn of the market, and buy or sell as its momentary complexion directs. A mere soldier of fortune, in the strictest application of the phrase, without political affections or social leanings, he wanted motives for any more long-sighted views than those which were connected with his own personal welfare; and if the warmth of his heart had attached him to any party or principle, his narrow uneducated intellect admitted not of that clear perception of cause and consequence, which sees the future in the present, which anticipates a still distant result, and leads by a pre-conceived path to its ultimate attainment.

Thus it is that he appears in the eyes of M. Guizot.

"When he was once engaged in an undertaking, a singular sagacity in discerning the true state of affairs preserved him from all uncertainty as to his immediate course. Seldom occupied with anything but the accomplishment of that which was required by his actual situation, he took a comprehensive view of it, and suffered nothing to escape him which might endanger the future. He was one of those practical men with whom reflection does not precede experience, but whom experience enlightens by degrees; and whose understanding, insensible to any truth which is not presented to them in a tangible form, yet seizes it so soon as in their eyes it assumes a place in the business of life. Monk, perhaps unequal to a complete preconception even of himself, was never wanting to the occasion; the entire scope of a great plan may never have presented itself to his mind, which was rather shrewd than capacious, and rather firm than enterprising; his sedate character was little open to the temptations of a bold undertaking, but boldness, when the emergency required it, was as easy to him as discretion; and when, as in proceeding step by step, ever sure of his footing, he reached one of those dangerous points wherein a great resolution is necessary at once to surmount great obstacles, his good sense, quick and confident, advanced straight towards the difficulty, however gigantic it might be; and the most daring course became, with him, the simplest, so soon as it was the most expedient."

In this estimate of Monk, we, to a certain extent, coincide, notwithstanding the elaborate note of Mr. Wortley, which, though it tends to rate him high amongh his class as a man of ability, yet offers nothing to warrant our removing him into a superior one. An attentive perusal of the volume before us has not altered, but rather confirmed the opinion we previously held, that the hero of the restoration, so far from having, at the death of Cromwell, any distinct apprehension of what was to come, had not even, when he crossed the Tweed at Coldstream, made up his mind as to the conduct he should pursue. Nay, notwithstanding the royalism of his brother and of Gumble, of whose desires he seems to have been the unsuspecting instrument, we cannot believe that, even till after the time of his attack on the city, he had any decided intention of overthrowing the existing government. The whole matter, however, in dispute, (it is, indeed, the one subject of the entire work with its annotations,) is perfectly valueless. Both as respects the revolution itself, and the character of Monk, it signifies not one farthing whether, supposing him to have had any long-sighted designs, he was treacherous to the king's party or to the Commonwealth,—whether he gave the former hopes which he had not at the time any intention of fulfilling, or whether, when he "publicly cudgelled an officer who said 'this Monk will at last bring in Charles Stuart,'" he was betraying the party who employed and paid him. In either case, he only justifies what his biographer declares of him,—namely, that he was one who "looks upon lying but as a stratagem of war, and upon fraud as a prize won from credulity." But a man capable of such sentiments would inevitably have sided with the strongest; and his having acted the part he did at the moment he did, we believe to have been a pure obedience to necessity, and a proof that the restoration would have occurred, had no such person as himself ever existed. There is, indeed, abundant evidence that his game was again and again on the point of being snatched from his hand, by other players, owing to his indecision; and that, at last, he was hurried on by circumstances, without being even in the slightest degree the master of them.

For our own parts, we are inclined to think that the absence of any fixed and settled purpose was essential to his success. Had he from the beginning entertained a determination to restore the monarchy, it seems impossible that he should not have been divined by the many interested parties which surrounded him. On this point, Mr. Stuart Wortley remarks:—

"The course of 'astonishing dissimulation,' in Mr. Hallam's words, which Monk steadily pursued from the time of his first declaration in Scotland to that of the Restoration, is such as to defy all ordinary perfidy, and to rouse every sense of common honesty in array against the admiration which is due to his dexterity. 'Cromwell,' Mr. Hallam adds, (ii. 389,) 'was a mere bungler to him.' Cromwell, indeed, had to gain the confidence of a single all-powerful party, which he accomplished by a system of adroit hypocrisy, uniform and sustained. Monk had to deal with a chaos of factions and intrigues; all struggling for dominion; all jealous of him and of each other; all straining to detect his purpose: he was compelled to humour those whom he despised, and to repel and discourage those whom he meant to raise;—he had to parry the indiscretion of his partisans, as much as the malice of his enemies;—and he had to depend on instruments which were scarcely under his control, and which might, by a single oversight, be turned to his destruction. Yet he contrived for a space of six months to keep all in dependence on his will,—to maintain his position as the centre of all hope and all anxiety,—and at length (to his credit it must be recollected) to accomplish his purpose without bloodshed or confusion. 'Even his extravagant adulators, although they do not, of course, record the strongest instances of falsehood, attempt no more than an excuse for it on the plea of necessity; and yet Clarendon (vii. 381) has the face to affirm that he was 'throughout his whole life never suspected of dissimulation!' An assertion, meant, of course, to disparage him, by showing that he had no hand in the Restoration, to which he was driven in spite of genuine protestations. A strange and perverse attempt! an attempt to injure the fame of an enemy, by proving that he was *not* a liar!"

† Φιλοσοφώτερον καὶ σπουδαιότερον ποίησις ἱστορίας ἐστίν. Ἡ μὲν γὰρ ποίησις μᾶλλον τὰ καθόλου, ἡ δ' ἱστορία τὰ καθ' ἕκαστον λέγει.—*De Poetica.*

Now we, so far, agree with Clarendon, that we cannot but think that the very surest way to gain a reputation for secresy, is to have no secret to keep. We do not believe, indeed, that Monk was free from dissimulation; but we confine it to that temporizing falsehood, which is all things to all men, in the hope of making use of each in his turn; and this, Monk would have been all the better able to do, if his conscience or his passions were never at hand to betray him. It is just possible, that, his own personal turn being served, Monk might have preferred the restoration, as the most settled order of things, and the least likely to place him in future jeopardy; but that is as much as we can concede, as to any remote aspiration towards bringing about that event.

From the course of these remarks, it will be clear to the reader, that this biography affords only a partial and limited view of the times to which it relates; but it is perfectly inconceivable how a writer of Mons. Guizot's turn of mind could have contrived so completely to avoid the whole philosophy of the subject. This he has done to an extent that leaves us utterly in the dark as to his design in undertaking the work. Monk, it is true, had nothing of the spirit of his age in his personal character, but its energy; and his biographer had no enforced occasion to touch either on the causes or the consequences of the greatest struggle for liberty which any nation has yet exhibited. But there is in Mons. Guizot's work so little of his characteristic generalization, so little even of common reference to the history of the times, that they who have it not at their fingers' ends, must be occasionally at a loss to follow the narrative: to the ordinary run of French readers it must be little less than a riddle.

Of Mr. Stuart Wortley's part in the book, we may observe, that the translation is well executed, in a correct, unpretending, and facile style; and that in the notes he labours to whitewash Monk as zealously, as if he had an hereditary or vested interest in his character.

A Romance of Vienna. By Mrs. Trollope. 3 vols. Bentley.

TWELVE months since, we were called aside from the grave proceedings of the British Association by the 'Vicar of Wrexhill.' The authoress of that charitable novel once again beckons us away from sections A, B, C, &c. &c., to introduce us to her 'Romance of Vienna.' Charm she never so *un*-wisely, we are compelled to listen. This romance, however, is but another chapter from *her* book of 'How to Observe.' Though its pages may not contain any scenes so objectionable as some in the 'Vicar,' they are throughout pervaded by the characteristic offences of that work—the same malicious and busy appeal to every narrow and bad prejudice. Having demolished, as she imagines, Dissent, in her 'Domestic Manners of the Americans,' and Catholicism, in her 'Paris and the Parisians,' and Calvinistic Protestantism, in her 'Vicar' aforesaid—Mrs. Trollope here sets herself to show up Judaism in Austria. An honest, a befitting vocation this, for an English gentlewoman!—a trade for which materials can never be wanting. We are the less inclined to mitigate the strength and earnestness of our former protest against such works, from a fear that there is, just now, some danger of their style being largely and successfully introduced into our fictions. With others, it may have been fallen into unconsciously, and is persisted in, out of habit, rather than malice prepense; whereas with Mrs. Trollope, it is a deliberate and settled purpose.

The story of her 'Romance' runs thus:—A young Austrian Count Von Alderberg, in the year 1815, marries a Miss Ringold, because he can obtain her on no other terms, and takes her abroad. There, satiety leads him to plan and to execute the concealment of this marriage. After having made an all but successful attempt to enlist Kaiser Francis in behalf of herself and boy, the lady is shut up by her amiable husband in a castle, belonging to his banker and "amatory agent" (we quote *verbatim*)—an Israelite, and blessed with a Sycorax of a mother, who spouts the language of the Prophecies on common occasions; the injured wife's attendants being Peppè, a peasant girl, with whom Count Alderberg first, and then the said Imla Balthazar intrigues,—and her mother rough and violent—but honest withal,—who takes Marie's part to spite her daughter. Further, the Count intends to make his wife over by way of bargain to the said Imla Balthazar, and only relaxes in his fiendish persecution, and only consents to leave her the guardianship of her son, on her binding herself by a solemn oath that she will never take further steps to prove the legality of her marriage.

Such is the first act of the drama: the second opens with *La Crême* of Vienna, into which the young Ferdinand de Ringold—though understood to be illegitimate—is introduced on the plea of his splendid appearance and the great scarcity of waltzing partners for a coming carnival. The small gossip, the ceaseless gaiety, the total want of intellect among this society, which Mrs. Trollope has elsewhere described as her Heaven upon earth, we doubt not, are pictured from the life. While a boy, Ferdinand has been domesticated in banker Balthazar's house, who is married to a rich and lovely Jewess. This poor Paria lady, whose listless life and neglected situation, Mrs. Trollope paints with *gusto*, rather than with indignation, takes to reading, for want of better interests—that is, the pleasures of *La Crême*—studies Shakespeare at midnight with young De Ringold—and ends somewhat *à la Francesca da Rimini*, by making violent love to the handsome young soldier; which he, no Paolo! rejects as frankly as it is offered. In fact, he has already become attached to a young Countess Oswold—and, for her sake, given a similar response to the passion of the Duchess of Clevemont, which is expressed (in the shape of a proposal of marriage,) as plainly, but more delicately than the lawless love of the Jewess. Whether these two rejected ladies help or hinder the progress of his suit, we shall not further disclose; nor yet indicate the scenes of a more animated and romantic nature, which, by a cleverly devised *da capo*, connect the opening with the catastrophe of the novel. We have told enough to justify our censure of the *animus* of this book: we must however in justice add, that it is cleverly written—full of lively scenes and elaborate descriptions: and we can only hope that so painful a combination of what is clever with what is evil may prove—

Like a brotherless hermit, *the last of its race.*

OUR LIBRARY TABLE.

Examples of Gothic Architecture, Part III., *The Manor House of South Wraxhall, Wiltshire, and the Church of St. Peter at Biddestone*, by T. L. Walker, Architect.—This is the concluding part of a volume on the Architecture of the Middle Ages, which Mr. Walker has published in emulation of similar works by his old and respected master, Pugin. We congratulate him on the completion of his task, which has been distinguished for taste in the selection and skill in the execution. The Vicar's Close, at Wells, the Manor House and Church at Great Chalfield, with the illustrations of the present volume, have been rescued from oblivion, and will now furnish examples to the student and professor; and this, too, in a style which is eminently useful, and capable of application to our domestic usages. We trust that this volume will not close the literary labours of Mr. Walker; we hope he will go into Oxfordshire, and some other counties, and enrich his folio with specimens taken from a more recent period of Gothic, and call public attention to some of those old mansions where the imposing character of the Gothic style is blended with more practicable and every-day arrangements and embellishments; sufficiently enriched to interest, yet not so loaded with florid tracery as to render its adoption beyond the reach of the simple country gentleman. The plates in this work are drawn and engraved with an accuracy and spirit which cannot be surpassed, as far as relates to their architecture; but some of the foliage and grotesque heads and figures are somewhat deficient in accuracy of drawing.

Nolan's Evangelical Character of Christianity.—The Rev. Dr. Nolan has assailed, with some asperity, that large section of the English clergy usually called Evangelical, declaring that their doctrines are opposed to Scripture, as interpreted in the thirty-nine articles, and that their practices are inconsistent with the discipline of the Anglican church. The tone of the book is sharper than we could wish, and while Dr. Nolan complains of the proneness of his antagonists to judge uncharitably, he too frequently pronounces condemnation on light and insufficient grounds.

Conversations on Metaphysics.—This little work was written by a French Jesuit, one of the few of his order who cultivated the rational philosophy of Locke and Bacon; he labours to remove the general prejudices against metaphysical speculations, by showing the limits which divide their use from their abuse. The translation will, we doubt not, be acceptable to all who have a taste for mental science.

Connexion of the East India Company with Idolatry.—This is an exposure of practices which have been often denied, but which are now, we fear, proved by official documents. It would seem that idolatry is profitable, and consequently favoured by the lords of Leadenhall Street.

List of New Books.—Robertson's Letters on Paraguay, 2 vols. post 8vo. 21*s.* bds.—Memoirs of George Monk, Duke of Albemarle, 8vo. cl. 12*s.*—Lindsay's Travels in Egypt, Edom, and the Holy Land, 2 vols. 8vo. cl. 24*s.*—Thomson's Chemistry of Organic Bodies, 8vo. cl. 24*s.*—Walkingame's Tutor's Assistant, by Taplin, new edit. 12mo. bd. 2*s.* 6*d.*—Morison's Counsels to Young Men, 3rd edit. 18mo. cl. 2*s.* 6*d.*—Memoir of Mrs. Louisa A. Lowrie, of the Northern India Mission, 18mo. cl. 3*s.* 6*d.*—Clarkson's Strictures on Wilberforce's Life, 8vo. bds. 5*s.*—Moore's Irish Melodies, 13th edit. fc. cl. 10*s.*—Marcet's Conversations on Land and Water, fc. cl. 5*s.* 6*d.*—Riddle's English-Latin Dictionary, 8vo. cl. 10*s.* 6*d.*—Riddle's English-Latin, and Latin and English Dictionary, in one vol. 8vo. cl. 31*s.* 6*d.*—Mayer's Sportsman's Directory, and Park and Gamekeeper's Companion, 6th edit. enlarged, royal 18mo. cl. 5*s.*—English History Tables, from the Invasion of Julius Cæsar to the Death of William IV., by C. H. Bateman, oblong folio, cl. 3*s.* 6*d.*—Ness's Antidote against Arminianism, by Jones, 6th edit. 18mo. cl. 1*s.* 3*d.*—Huntington's Select Works, 6 vols. 8vo. cl. 3*l.* 18*s.* 6*d.*—Trollope's Romance of Vienna, 3 vols. post 8vo. 31*s.* 6*d.*—Fores's Coronation Procession, cl. 31*s.* 6*d.*—Chitty on Pleading, Part II. royal 8vo. bds. 16*s.*—Laurence's Perspective Simplified, 8vo. cl. 7*s.*—Gibbons on the Law of Dilapidations and Nuisances, 8vo. cl. 9*s.*—The Last Days of Aurelian, or the Nazarenes of Rome, 2 vols. post 8vo. bds. 12*s.*—Curiosities of Literature Illustrated, by Bolton Corney, post 8vo. bds. 7*s.* 6*d.*—De Porquet's First Italian Reading Book, 2nd edit. fc. cl. 3*s.* 6*d.*—De Porquet's Petit Vocabulaire and French Genders, in two colours, fc. cl. 3*s.* 6*d.*—De Porquet's Petit Vocabulaire Français-Anglais, fc. cl. 2*s.* 6*d.*—Hand-Book of Archery, 24mo. cl. gilt 1*s.*—The Zoological Gardens, with 53 illustrations, 12mo. cl. 3*s.* 6*d.*—Probus, or Rome in the Third Century, 2 vols. 12mo. cl. 12*s.*—Wells's Act for Abolishing Imprisonment for Debt, with explanatory Introduction, Notes, and Index. 12mo. swd. 2*s.* 6*d.*—Stephens's Incidents of Travel in Russia, Turkey, &c., 2 vols. 12mo. cl. 12*s.*—Nordheimer's Hebrew Grammar, 8vo. cl. 15*s.*—Lingard's History of England, Vol. VII. 12mo. cl. 5*s.*—Burton, or the Sieges, a Romance, 2 vols. 12mo. cl. 10*s.* 6*d.*—Mitchell's Game Book, with illustrations, by Henry Alken, oblong folio, swd. 8*s.* 6*d.*—Questions and Answers deduced from Goldsmith's History of Rome, 12mo. cl. 2*s.* 6*d.*—Recollections of a Tour in the North of Europe, by the Marquis of Londonderry, 2 vols. 8vo. 24*s.* cl.—Three Expeditions into the Interior of Eastern Australia, by Major T. L. Mitchell, 2 vols. 8vo. 40*s.* cl.—History, Antiquities, &c. of Eastern India, by M. Martin, Vol. III. 8vo. 21*s.* cl.—Duty and Inclination, edited by Miss Landon, post 8vo. 31*s.* 6*d.* bds.—Lardner's Cabinet Cyclopædia, Vol. CVI. "British Biography," Vol. III. fc. 6*s.* cl.—Bosworth's Anglo-Saxon Dictionary, royal 8vo. 42*s.* cl.—Robert Hall's Sermons, Essays, and Reviews, new edit. 12mo. 5*s.* cl.—Medhurst's China, 2nd edit. 8vo. 12*s.* cl.—Skinner's Overland Journey to India, 2nd edit. 2 vols. post 8vo. 12*s.* bds.—Johnstone's History of British Zoophytes, 8vo. 30*s.* cl.

EIGHTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

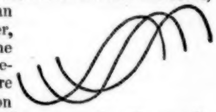
[*From our own Correspondents.*]

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.—TUESDAY.

The first paper read, was 'An Account of a Level Line measured from the Bristol Channel to the English Channel, during the years 1837-8, by Mr. Bunt, under the direction of a Committee of the British Association,' drawn up by the Rev. W. Whewell, one of the committee.

At former meetings of the Association it had been considered desirable to determine the relative level of points on the coasts distant from each other, and of the sea. A sum of money was placed at the disposal of a committee for this purpose, and after the meeting at Bristol, in 1836, those members of the committee who had the opportunity of conferring with each other, took upon themselves the task of directing the execution of the plan. Mr. Bunt was selected to perform this service, and was provided with proper instruments, made by Simms. The line selected was from Axmouth, on the south coast of Devon, to East Quantockhead, Wick Rocks, and Portishead, on the north coast of Somersetshire. The levelling between Axmouth and Wick Rocks was performed in 1837. Tide observations were made at various periods up to July last, since the permanent level of the sea could only be determined by knowing its changeable positions. The first result was, that the level of the *mean water* at Axmouth and Wick Rocks agreed within a fraction of an inch, and that the level of mean water at Portishead was 4½ inches lower; but, by subsequent examination, there appeared reason to believe that the observations at Axmouth were vitiated by being made within the bar at the mouth of the river. Hence simultaneous observations were made at Axmouth and Portishead in July last; the result of which was, that the level at Portishead is ten inches higher than that at Axmouth. According to this result, the mean level at Wick Rocks is 3.8 inches *higher* than at Portishead. In the course of these observations there appeared to be some undiscovered cause of error in the levelling operations,† by means of which the lower end of a level line was apparently thrown down. This effect was about one inch in ten miles, in most of the observations, and since it seems highly probable that it arises from some constant cause, and that therefore the mean of the extreme results would approach very near the true level, Mr. Whewell considers that the result may be trusted within an inch or two. Mr. W. offers no account of the cause of this apparent error. The result to which Mr. W. is led, is, that *the mean tide must be taken as the level of the sea.* This result had already been arrived at by Captain Denham and Mr. Walker, but only by observations at one place in each case (Liverpool and Plymouth), but is now confirmed by evidence of a kind quite different. Mr. Whewell observes further, that this result is of great practical consequence, since, if the mean water had the same level all round the island, this mean level may be used for engineering purposes, (as in the planning and construction of canals,) and that it appears very desirable to extend such a comparison to other points, at the principal ports of the empire.

Prof. Lloyd did not rise for the purpose of throwing the least discredit on the valuable labours of Mr. Bunt, or of controverting the principal conclusion at which Mr. Whewell had arrived from these observations: but he thought it but fair to the gentlemen of the Royal Engineer Corps, engaged in the trigonometrical survey of Ireland, to state, that the conclusion to which they had come was decidedly different from that of Mr. Whewell, and that while according to their observations the line connecting two distant points of mean tide height differed by feet from a level line—that, on the other hand, which joined two distant points of average low water differed but by inches from a level line; of this fact, their observations gave clear indications on the shores of Lough Foyle and Lough Swilly, which approached each other at one part within, he believed, seven miles, while their mouths communicated with the open sea in very different directions.—Mr. Whewell replied, that he did not consider that observations on the level of the positions of mean tide in two such localities as Lough Foyle and Lough Swilly, would give results much to be depended on. He, himself, was at first of opinion that even the Bristol Channel was too confined for much dependence to be placed on observations made there on the tides, and it was not until he had satisfied himself, by long-continued and minute attention to the phenomena of the tides on its shores, and of the constancy of their indications, that he had ventured to draw the conclusion to which he had come; and that in one or two instances he had abandoned stations, as unfavourable for these observations, simply from the changing of the phenomena which had taken place from local causes during the very progress of his observations, as, in one instance, the forming of a bar of sand across the mouth of the harbour. Since he had come to the Association, he had seen in the last printed volume of Reports a statement which he conceived to be strongly corroborative of his view in the tide observations made by Mr. Russell, in the Clyde. The tide wave, as it passed up the river, was, by the form of the channel and local causes, forced to ascend and alter the level, both of the place of mean tide and of low water, which he described by the aid of a diagram, something similar to that here presented.



—Dr. Robinson had not any intention of controverting the results obtained by Mr. Whewell. On the contrary, he believed it to be established that the line connecting the mean of high and low water at Portishead and Axmouth was a line of level; but he thought it was not in the true spirit of inductive philosophy to assume the generality of a law from one solitary fact. It was impossible to set aside so summarily the statements made by the officers of the Irish survey, and he regretted exceedingly that none of them were present. In the case stated by Prof. Lloyd, an anomaly like that mentioned by Mr. Whewell might possibly exist. Lough Swilly was an estuary, wide at the mouth, gradually contracting, and with deep water; Lough Foyle narrow at the entrance, and with a bar, a wide expanse within, and much of it shallow, while it received a pretty large river. But he understood from the officers with whom he had lately corresponded, (while determining from the data which they gave the height of his own observatory above the sea,) that similar results were derived from simultaneous observations of depressions and elevations, connecting points of the Irish shores, where no such disturbing circumstances appeared. The assuming "low water of ordinary spring tides" as the level of the undisturbed ocean appeared so strange, that he put the question, "spring tides, with what declination and what parallax?" and was informed that the points which related to his inquiry were determined by the average of a year. Under these circumstances, he could not but consider more inquiry desirable, and he hoped Mr. Whewell would prosecute it further. It could not be in better hands; and though the expense might be considerable, there was not a member of the Association who would hesitate when such knowledge was to be weighed against gold.—Mr. Greenough pointed out a locality where he thought a peculiarly favourable station might be selected for running levels to points of the ocean very distant, and for settling the present dispute; he alluded to the course of the great south-western railway.—A gentleman in the Section, whose name we were unable to learn, pointed out the Bay of Fundy as a locality well calculated for tide observations and levels; for it was well known that at the bottom of the bay, and at one side, from various causes, the tides rose to most extraordinary heights, while at other places in the neighbourhood the heights of the tides, and other phenomena, were not peculiarly remarkable.—Sir John Herschel said, that from the extraordinary rise of the tide at the Bay of Fundy, and from the phenomena of other localities, it was quite clear that neither could the connexion of the points of mean tide, nor those of low water, universally give a level surface. In fact, if he recollected rightly, the tides in the Frith of Forth demonstrated the point; for the tide stood so high at Alloa at the time of flood, that if the same level wave ran down to Leith, most of the houses of that port would be laid under water, whereas the total rise and fall of the tide either at Alloa or Leith was no way remarkable; from this it would appear that a person could no more deduce arguments from either of these points of the height of the tide, in favour of level lines, than he could by observing a wave splashing about over a rough road. And he feared, that if even the tide observations had arrived at that state that the rise and fall in the open ocean could be deduced, yet even there the positions either of low water or of mean tide would be so much influ-

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† As this question is one of general interest and great importance, we think it advisable to give the letter addressed by Mr. Bunt to Prof. Whewell:

"Previously to my commencing the series of Levels, on which I have been recently engaged for the British Association, between the English and the Bristol channels, under the directions of Prof. Whewell, I was desirous of deriving such assistance as might be obtained from any published account of a similar undertaking, in which due attention had been paid to all the niceties which are required when the attainment of great accuracy is of importance. All the ordinary treatises on Levelling that I have seen are merely elementary and superficial; and the only account I have been able to meet with containing such information as I wanted, is that given by Capt. Lloyd in the Phil. Trans. for 1831, which details with great clearness, and at considerable length, every particular connected with his levelling from Sheerness to London, a scientific undertaking of a similar character to that in which I was then about to engage. I am indebted to this memoir for much valuable information and assistance, and have either adopted or imitated several of Capt. Lloyd's arrangements, most of which appeared to me to be judicious and useful. There is, however, one important point on which I differ from him in opinion;—to to this I shall have occasion to advert presently.—[Mr. B. then described his instruments and the manner of using them, and the common causes of error, and states that he should not consider it safe to depend on a single course of levels, whatever may have been the precautions employed to guard against error.]—The total length of my line of levelling between Portishead, near Bristol, and Axmouth in Devonshire, is seventy-four miles, besides the branch lines to East Quantockhead and Bristol, which amount to about twenty miles more. This distance was divided into stages of an average length of ten miles, each of which was twice levelled over, first in one direction, and then in the opposite, before the next stage was commenced. It is very remarkable, that, with a few partial exceptions, the heights of all the points touched upon by both series of levels, came out *less* by the series *returning*, than by the series *going;* the first station, or starting point, consequently, always appearing lower when I returned to it, than when I set out from it. But, as the height of this point is, of course, considered the same in both instances, the error must be thrown on the distant point, or that at which the return commenced, by adding the amount of error found on arriving at the first station, to all the heights in the returning series, which reverses the order of the differences, and makes the heights in the second series rise progressively above those in the first; both series being now reckoned from the first station, at which the error becomes = 0.—[A table of differences is then given.] After the most careful and patient examination of every minute circumstance which could possibly tend to occasion these curious differences, I am of opinion that they arise principally from rapid variations in the amount of atmospheric refraction, and that the cause of the errors taking one direction, is in some way or other connected with the progressive changes of average temperature in the course of the day, from about eight in the morning till six or seven in the evening, the usual limits of my working hours. These variations of refraction are much greater and more sudden in summer (the season in which this work was principally executed,) than in the winter; especially during the forenoon of a hot sultry day, when there are frequent alternations of cloud and sunshine, and copious exhalations of moisture from the ground, indicated by the rapid undulation of any object seen in the telescope. On such occasions I have sometimes known the sudden clearing away of a cloud from the sun followed almost immediately by a change in the apparent height of the wave of one-sixteenth and even one-tenth of an inch on a distance of eighty-eight yards. At other times, the change has been less rapid, and several successive readings have all either gradually increased, or gradually diminished. Different seasons or states of the atmosphere may therefore be quite sufficient to occasion a more rapid increase of these differences, at certain times than at others. For this reason it appears to be much better to divide the distance into stages, and finish them one at a time, than it would be to level the whole distance in one direction, before returning over any part of it: it being much more probable that errors dependent on the atmosphere will balance each other in the former than in the latter case. My own experience, therefore, necessarily leads me to conclude, that no levelling can be expected to give a correct result, *unless it be performed in opposite directions, and the mean of both results be taken;* instead of depending, as Capt. Lloyd appears to have done, on the consistency of separate sets of successive readings. I have myself invariably found the agreement of these to be almost identical, both those of the series going, and those of the series returning, notwithstanding the great progressive difference of these two series of levels from each other; of which progression not the smallest trace is discoverable in the separate columns of the same series. I have entered the more minutely into this subject, because I am not aware that any one has described, or even noticed the existence of such differences before; and should feel much interest in reading the statements of any experienced person who had been engaged in a similar undertaking, and had conducted it with sufficient care to render the *law* of the errors in any degree discernible."

enced by the shape of the bottom of the ocean as to be types rather of that shape than general indications of the direction of gravity at the surface. He exemplified his views by a reference to the curious tides of False Bay, Cape of Good Hope.

Prof. A. D. Bache, of Philadelphia, then communicated a 'Note on the effect of Deflected Currents of Air on the quantity of Rain collected by a Rain-gauge;' the more remarkable phenomena noticed in it being represented by diagrams.

As the experiments referred to grew out of a report made at the request of the British Association, I conceive it proper (said Mr. Bache) to communicate to the Section a conclusion to which they have led me. The first of the reports of Prof. Phillips, on the quantity of rain collected at different heights, which was presented at the Cambridge meeting of the Association, had interested my friend Prof. Rogers, then present, and at his instance I commenced a series of observations about the close of the year 1833. Philadelphia, from the extent of the plain on which it stands, is a good locality for such a purpose. The observations were at first made by gauges placed at three different heights. One of these stations was the top of a tower formerly used for making shot. The height of the tower is 162 feet. A second was near the ground within the inclosure about the tower, and the intermediate one was the roof of the University. My attention was, however, ultimately fixed upon the fact that the effect of eddy winds upon the phenomena observed, was by no means a secondary one in amount, and that I could not hope to deduce a law, or to throw any light on the nature of the phenomena until this disturbing action was got rid of. This is not the place, nor have more pressing engagements allowed me the time to discuss the numerical details of these observations, but I have thought, notwithstanding, that it might be useful to those who may undertake similar experiments, to submit some of the evidence of the effects which I attribute to deflected currents of air. The observations on this point were chiefly made at the upper station, on the top of the tower. The tower is square in its section, and the alternate sides are nearly parallel and perpendicular to the meridian. At the roof the horizontal section is about twelve feet on a side, and a parapet wall, cut like a battlement, surrounds it. At first, one gauge was placed at the N.W. angle of the tower, rising about six inches above the parapet wall; subsequently, a gauge for collecting snow was placed at the S.W. angle; and ultimately, four gauges, besides the original one, were placed at the four corners of the tower, upon the parapet wall, above which they rose about ten inches. The rain gauges consisted of an inverted cone, with a cylindrical rim, about five inches in diameter, attached to the base, and a small aperture near the vertex. This fastened tightly upon a vessel serving as a reservoir. The snow gauges were frustums of upright cones, the upper base being nearly four inches in diameter. The water was measured in a glass tube, in which one thousandth of an inch of rain fallen was measurable. When the snow gauges became useless, they were used as rain guages, by attaching a funnel to them, or were finally replaced by rain gauges similar to those described. Further details may, I believe, without inconvenience, be postponed. The quantity of water collected was measured after each rain, and the direction of the wind during the rain was frequently noted. To illustrate the effects which I attribute to currents of air deflected by the tower, I have taken from the journal of the latter months of observation, the records of the quantities of rain collected by four similar gauges, placed at the four angles of the tower, under different circumstances as to the direction of the wind. These are selected so as to present, as far as possible, a case of rain with each principal direction of the wind.

Date.	Wind.	Angle of the Tower at which the gauge was placed. Rain in Inches.				Relative quantities at different angles.			
		NE.	SE.	SW.	NW.	NE.	SE.	SW.	NW.
July 26	N	0.552	0.760	0.749	0.583	1.00	1.37	1.35	1.05
Aug. 6	NE	0.311	0.378	0.607	0.491	1.00	1.21	2.08	1.58
July 15	E & N by E	0.912	1.398	1.868	1.715	1.00	1.53	2.04	1.88
April 13	NE, SE, SW	1.316	1.186	1.568	1.670	1.10	1.00	1.31	1.40
Aug. 26	S & SSE	0.407	0.253	0.241	0.391	1.68	1.04	1.00	1.62
June 19	WSW & SSW	0.380	0.285	0.252	0.198	1.06	1.43	1.26	1.00
Sept. 1	W	0.302	0.328	0.202	0.141	2.14	2.32	1.43	1.00
Sept. 5	WNW, N	0.638	0.731	0.429	0.679	1.48	1.70	1.00	1.58

On this table I would remark:—1. That it illustrates the very great differences between the quantities of rain collected at the different angles of the tower. In one extreme case the quantity collected at the S.E. angle, was 2½ times that at the N.W. angle. 2. That, in general, the gauges to leeward received more rain than those to windward. Thus, with a north wind, the gauges at the S.E. and S.W. angles received more rain than those at the N.E. and N.W. angles. With a N.E. wind the gauge at the S.W. corner of the tower received the most rain. In the case given in the table, the ratio of the quantities is nearly 2.1 to 1. With an easterly wind the N.E. and S.E. gauges received less than the N.W. and S.W. With a south-easterly wind the S.E. gauge received the least, and the N.W. the greatest quantity of rain, and so on, nearly in the order stated in the general remark. 3. As the more considerable rains accompany certain winds, it is not to be expected that averages of any number of observations exposed to such errors will lead to an accurate result of the quantity of rain falling at a certain height above the surface. In fact, the averages, from a period of nine months, do not agree nearly so well as those from the selected specimens in the table. These give ratios of 1, 1.19, 1.24, and 1.20, for the quantities at the different angles, while the former mentioned averages at the N.E. and S.W. angles, are nearly as one to one and a half. 4. The connexion between the direction of the wind and these effects is easily made out; but without an anemometer this is not possible for that of the force. I have found, however, in the case of the N.E. wind, which most frequently attends our greatest rains, considerable differences, even with a moderate wind; amounting, for example, as high as a ratio of one and a half to one. Having seen that I could not hope for accurate results by these arrangements, I next tried the effect of elevating the gauge upon a high pole, as was done by Prof. Phillips with the gauge on the top of York Minster. The differences that appeared in this case were very trifling indeed; thus, on the 26th of August, when the NE. and SW. gauges upon the parapet wall gave quantities in the ratio of 1 to 1.68, those six feet above the parapet gave 1 to 1.08; with a more moderate wind the quantities were more nearly the same. If these conclusions apply equally to the other stations where my observations were made, I must have also changed the arrangement of the gauges there, to prevent their exposure to the effects of deflected currents, before I could have deduced a law for the quantities falling at different heights. Absence from my country has prevented me from pursuing further this inquiry, which I shall gladly resume on my return, if circumstances are favourable to my so doing.

Dr. Robinson said, that some years since he had placed on the roof of the Armagh Observatory a rain gauge, and registered it with some interest; but he soon found that in high winds it indicated less rain than the quantity which obviously fell, and his ideas of the cause of this error were confirmed by watching the instrument during a heavy shower and a violent gale of wind. The drops were plainly seen, raised by the eddy, and careering over the funnel, without one drop entering it. The shower lasted many minutes, but no measurable quantity of water was found in the gauge. This lessened his zeal for such observations, but it led him to consider whether there was not a fallacy in measuring rain by the quantity received on a horizontal surface. If wind blows, what surfaces receive the falling rain? certainly vertical rather than horizontal: walls, hedges, trees, stems of grass, &c. Would not the following arrangement give a truer measure—a sphere of known surface, supported by a slender stem which would receive the rain, and let it descend till capillary attraction ceasing to support it, it might drop into a small funnel below, encircling the stem at a proper distance, and be conducted into the usual reservoir? He had no doubt that such an instrument would give a far truer approximation to the real quantity which falls than any now in use.—Prof. Stevelly said, that at least two very essential elements, affecting the quantity of water received in our rain gauges, had been overlooked; one affected even open places where there were no obstructions, the other the case referred to by Prof. Bache, where the wind met with obstructions in its onward progress. Any one who had ever observed a shower at a distance, must have noticed, that though the drops of rain as they descend, appear at a distance like lines or threads, yet the several threads were by no means perpendicular to the horizon; but since the mouths of our rain gauges were all set parallel to that plane, it was obviously of great consequence at what place in the descending shower the mouth of the gauge was placed, for if placed where the lines in which the drops descended entered it perpendicularly, a large cylinder of the descending shower would enter the mouth of the gauge, but if placed at that part at which the lines in which the drops were descending were oblique to the plane of the mouth of the gauge, only a comparatively narrow pillar of the descending rain would enter. Now, as the natural obstruction of passing along the surface must cause the wind generally to move near the earth in a less rapid course than at a greater elevation, therefore generally the rain will fall in a more perpendicular direction near the earth than at an elevation, and cause the greater quantity of rain to be deposited at the lower station; and this he always felt was a circumstance that had been too much overlooked in deducing the conclusions from the registries kept by Prof. Phillips, of the quantity of rain received by gauges placed at various heights at York Minster, the conclusion derived being, that condensation of more water upon the drops was going on as they descended; whereas he conceived, that in general the effect would be opposite, as even while rain is descending, the region near the earth is generally so much above the dew point, that evaporation instead of condensation is going on. As to the cause of the various quantities of rain received at the several corners of the top of the tower, as described by Prof. Bache, it is obvious, as any one may convince himself who watches the smoke coming out of a factory chimney on a stormy day, that obstructions placed in the way of the wind, such as towers, houses, chimneys, hills, &c., by stopping the wind on the windward side, cause an ascending current of air, which has already, as it came on, deposited its rain; and this, as it goes over the windward side of the top, wards off much of the rain that otherwise would be received in a gauge placed there; while on the lee side of the obstruction, the eddy there caused by the diminution of the tension of the wind, draws in a large funnel-shaped body of the air, with all its rain, thus increasing the quantity there received; and as you go round the tower in azimuth, between the windward and lee side, the quantity of water received will vary within these extreme limits. This cause also affected the validity of the conclusion from the registries kept at York, as Prof. Bache had already pointed out; but it had been apparently overlooked.—Colonel Reid always considered the quantity of rain falling at the same time, and in widely distant stations, as one of our most important meteorological elements; but, as the indications of the rain gauge

itself were now shown to be subject to great variations, it was advisable that every person acquainted with the subject should bestow attention on the best means of improving the construction of the instrument, or on the circumstances essential to the deduction of the true quantity of rain which falls at a given place, on a given surface, in a stated time.—Mr. Lloyd had lately heard from a friend a fact strongly illustrative of the effect of eddy currents of wind on the quantity of rain falling on particular spots. His friend, when on a mountain, which, although pretty flat, or a table land, on the top, sloped precipitously on one side, had been enveloped in a heavy fall of rain; but observing that the cloud did not extend below the brow of the cliff, he approached the most precipitous spot, and sat down on the very edge, and discovered, that within about three or four feet of the face of the precipitous cliff, not a drop of rain was falling, althoug hvery close to where he sat it fell in great quantity; and, on looking up, he could see it carried over his head by the eddy current.—Sir John Herschel was much gratified to find that meteorological subjects were everywhere beginning to meet with that portion of the attention of philosophers which they so well merited. The thanks of all interested in these pursuits were due to Prof. Bache for bringing this important circumstance under their notice, and, indeed, he might add, for the zeal and effect with which he and some of his countrymen were now pursuing meteorological inquiries. The rain gauge, in its present form, or at least as at present used, was obviously almost useless, since its indications, by even a change of position of only a few yards, are found to be so materially varied. The effect of these eddy currents on the large and small drops of the same shower must also be different, for the small drops would have their courses more deflected than the large drops, and these, as described by the diagram drawn by Mr. Stevelly, would enter the gauge in less quantity. Another effect of these eddies would be, to cause the drops, by clashing in their courses, to coalesce, and thus form large and less easily deflected drops. One fact, which particularly arrested his attention during his residence at the Cape, was the manner in which the south-east winds bring up the vapour of the sea. On the windward side of the Table Mountain, the clouds were spread out and descended very low, but frequently without any rain falling, while on the lee side they poured over the precipitous face of the mountain, producing, as they were rolled out, the well known phenomenon of the table-cloth. As he walked under tall fir trees in that neighbourhood, while those clouds were closely overhead, he was often struck with finding a heavy shower falling under them, while not a drop was outside; the fine filamentary tops of these trees, which, under usual circumstances, caused them to act as *parapluies*, here, where these tops became enveloped in the clouds, reversed their action, and afforded you a very plentiful wetting if you went under them.

Dr. Daubeny then read a paper 'On the Climate of North America.'

The Doctor began by observing, that although the general fact was admitted that the eastern portions of the New World possessed a lower temperature than the western portions of the Old, yet that much remains to be done before the relative climate of these two portions of the globe can be regarded as in any degree determined. In proof of this, he exhibited a table, in which he had entered a series of all the mean temperatures of different places in North America, which he had been able from various sources to collect, and showed that the greater part of them were very little to be relied upon as to accuracy. In Canada, the best observations yet made were those by M'Cord, of Montreal; and, in the United States, those communicated by the Regents of the University of the State of New York, with respect to the mean temperature of no less than thirty-three places within the state, where academies supported at the public expense are established. But the observations are likewise defective, in not taking any account of the intensity of solar radiation, which probably affects the distribution of plants and animals in a manner which is quite distinct from its accompanying temperature. Hence, though many plants which grow in this country are killed by the winters of comparatively southern latitudes in America; yet others, which require the warmth of a wall or of a southern aspect here, are found in comparatively high latitudes in the New World. But though the observations yet made are so imperfect, there seems no want of disposition either in Canada or in the United States to contribute to the advancement of meteorology, and to adopt the suggestions of European philosophers on this subject, as is evidenced by the promptitude with which Sir John Herschel's suggestions, with respect to hourly observations on certain days, have been acted on in both countries; and, hence, Dr. Daubeny suggested that it would be likely to contribute much to the advancement of this science, if the Association were to circulate extensively in the United States instructions both as to the use of meteorological instruments, and as to the proper hours for observing; and if they were to present to three or four public institutions in Upper and Lower Canada sets of the instruments deemed most important, carefully compared with each other, or with a uniform standard.

Sir John Herschel said, that of all the sciences which now engross the attention of the thinking part of mankind, none required a greater union of exertion than meteorology; in fact, from want of attention to this, there was no science in the pursuit of which so much time and labour had been thrown away. In it, union might emphatically be said to be strength, while mere individual exertion was little better than inaction. With this conviction, he had some time since ventured to propose that meteorological observations, continued through the twenty-four hours, should be made simultaneously in all parts of the globe. He was happy to say, that, in conformity with this suggestion, he had received numerous communications, giving the observations made at widely distant stations on the appointed days; and to none had he to express his obligations in stronger terms than to the philosophers of the United States of America. In some, however, of the most valuable meteorological registers, he found the hours of observation selected were those only of the day. Now, he was aware of the great additional labour required for night observations, and that nothing but zeal of a high-philosopher-power could enable observers, for any length of time, to pursue such observations; but when he considered the importance of these registers to the science, he could not but press the practice anxiously on public attention. The results of these night observations would be found to differ very widely, and often most materially, from those of day; the fluctuations of the barometer were different, the formation and dispersion of clouds and the falling of rain, all followed different laws by night from those which by day controlled their courses.—Sir David Brewster called attention to the important fact, clearly established by the meteorological observations recorded in the neighbourhood of New York, and those of Hansteen and Erman in Siberia, that two points of maximum cold existed in these regions, very generally agreeing in position with the centres of maximum magnetic intensities; and like them, too, the maximum of North America indicated a decidedly higher degree of cold than that which characterized the Siberian pole. Also, that the lines of equal mean temperature, as they surrounded these poles, had such a relation to the lines of equal magnetic intensity, as to point out clearly some yet unknown connexion between these two classes of phenomena.—The same gentleman who addressed the Section at the close of Mr. Whewell's paper, said, that he could not agree to some of the conclusions at which Dr. Daubeny had arrived. As to the connexion between animal and vegetable life and climate, something more would be found necessary than mere mean temperature. He had often ridden violently and used much bodily exertion in New South Wales, with the thermometer at 110° in the shade, when the same temperature in England would be insupportable; and in the East Indies all Europeans were so enervated when the thermometer stood at this height, as to be nearly incapable of active exertion. As to vegetation, we had on the one side of the Himalayan range, at an elevation of little more than 10,000 feet, lichens, and all the stunted vegetation of the polar regions; while, on the other side, at an elevation of nearly 16,000 feet, we had corn fields and large forest trees, and all the productions of temperate regions of the earth. Nor could he agree in Dr. Daubeny's conclusion, that the mean temperature to the west of the Alleghany mountains was much lower in North America than to the east. In his opinion, the contrary was the fact in many parts west of the Rocky Mountains. In California, and along the Columbia river, were found large cedars and other productions of countries bordering on tropical; while to the east, in the very same latitudes, could only be found lichens and other almost polar vegetables. In his opinion, the courses of rivers and of extensive forests, as well as of high ranges of mountainous tracts, were to be taken into account, as influencing most materially the climate of the circumjacent territories.—Dr. Daubeny explained that he had been misunderstood, if supposed to say that all places to the west of the Alleghany mountains were colder in their climate than those to the east; his observations had reference to the space included between the Alleghany and the Rocky ranges of mountains, or what is in part called the valley of the Mississippi, compared with the more eastwardly portion of North America. With many of the other observations of the gentleman he concurred, nor was he aware that they were opposed to any of the statements comprised in his very brief notice of the climate of North America.—Prof. Bache, of Philadelphia, made some remarks on the importance of connecting the observations making in the United States, with any which the British Association might institute in the colonies of Britain in North America. Considerable progress, he said, had, within a few years, been made in America in the science of meteorology. The abstracts of the reports of meteorological observations from the academies of the state of New York, and the deductions made from them by Sir David Brewster, had been a great stimulus to increased activity in that department. The recommendations of Sir John Herschel had not only been adopted by individuals, but had led to the formation of societies for the cultivation of meteorology. These, independently of other facts, convinced him that he hazarded nothing in promising the hearty concurrence of meteorologists in the United States in any extensive plan which the British Association should sanction.

Prof. Stevelly, one of the Secretaries, then read the following communication from Prof. Powell, 'On some Points connected with the Theory of Light.'

The very extensive investigations connected with the theory of light, and those parts of experimental optics which have the most direct bearing on the verification of such a theory, are, from their nature, only capable of being successfully prosecuted by the continued accumulation of results by different inquirers, which it is always an important part of the object of this meeting to bring together and to discuss. Accordingly, at previous meetings, I have laid before the Physical Section, from time to time, notices of whatever I had been able to effect myself in aid of such inquiries, or remarks on the most important labours of others in the same field. Though circumstances prevent my personal attendance on the present occasion, I trust the Section will allow me to communicate, in writing, a few remarks, which I hope may be subservient to the same end I have had in view in former communications. At the last meeting I dwelt upon the importance of extending observations on the refractive indices for standard rays, to more highly dispersive media. I have been anxiously looking out for specimens of such substances in a state capable of prismatic examination: I have as yet only obtained (in addition to those before examined) a prism of chromate of lead. On trial, I am obliged to report that the nature of the substance is such as I fear will preclude all determination of indices: the spectrum which it gives is confused and distorted; none of the lines are visible; and the whole of the blue and violet rays are absorbed. In the comparisons which have been made with *theory* by Mr. Kelland and myself, a particular discrepancy has always been found in the ray G. It is worthy of notice, that, with respect to this particular ray, there appears to exist some ambiguity in the *experimental data*. The part marked G by Fraünhofer consists of a multitude of small lines, which, in the more highly dispersive media, are spread over a considerable space. To *which* of these does his determination of the wave-length apply? In the figure in Sir J. Herschel's 'Treatise on Light,' the

letter G is affixed to a small line beyond the mass of these. Fraünhofer himself says, "at G many lines are accumulated." [Memoir, Edin. Phil. Journ. No. 18.] To this point, among others, I am now directing my attention, in a series of more accurate repetitions of my former approximate measurements, in which I am engaged for some of the more important media. I shall most thankfully receive, and examine, specimens of highly dispersive liquids, with which any scientific friends may favour me: the smallest quantity will suffice. I rejoice to observe that Mr. Potter has recently resumed some of his former subjects of inquiry bearing on the undulatory theory. I hope that at this meeting some discussion will be elicited on the question at issue respecting photometrical measurements, especially in connexion with the remarks of Prof. Lloyd in his Report (Brit. Assoc. Rep. 1834, p. 345); and particularly in reference to that primary point—the power of the eye to judge of the equalization of lights, and how far the respective illuminations of two spaces, in juxtaposition, may influence each other. To show the great uncertainty which exists on these points, I may just refer to the very simple experiment of receiving, on a white screen, the rays of a candle, and intercepting a part of the light by a clear glass plate, in which case the eye cannot recognize the least difference in the illumination of the part covered by the glass. Yet both at the first and second surfaces of the glass, reflections take place, which must manifestly abstract a very considerable portion of the light. At the first surface alone, (according to Mr. Potter,) not less than nearly *one half* the rays are reflected, at an incidence nearly perpendicular. Thus the lights received on the screen ought to be found nearly as two to one, instead of being equal. With regard to the mathematical theory, my attention was some time since called to the very able papers of Mr. Tovey, and particularly to that on Elliptic Polarization. (Lond. and Edin. Phil. Mag.] All the researches of M. Cauchy, Mr. Kelland, and the first of Mr. Tovey, for the integration of the differential equations for waves, in the form which includes the dispersion, proceed on the assumption that *certain terms vanish;* and this appears essential to the *general* solution. Mr. Tovey has, however, since shown, that if these terms do *not* vanish, we still have a *particular* solution; and this applies to the case of elliptic polarization. I have recently endeavoured to clear up some points connected with this inquiry. The particular case referred to is thus excluded, in all the former investigations, which are consequently imperfect. Upon the evanescence, or non-evanescence, of these terms, simply depends the elliptical and circular, or rectilinear character of the vibrations. Corresponding to these mathematical conditions, are those of the arrangement of the etherial molecules in the medium, or part of the medium, where the polarization is communicated. I have pointed out the connexion between these views and those of Prof. Macculagh, in which he connects with certain equations of motion, the elliptic polarization in quartz, by which Mr. Airy had explained M. Biot's results and laws. I do not enter into details, as the investigations will shortly appear in the Philosophical Transactions; and should Mr. Tovey happen to be present, he will no doubt be able to give some account of the subject. I will not further trespass on the time of the Section, than merely by repeating my hope that these imperfect notices may at least serve the purpose of eliciting some further statements on these topics, which I doubt not many members of the Section will be most able to give, and of calling forth that discussion which is always so valuable a means of promoting scientific truth, and the encouragement of which forms so important a part of the object of these assemblies.

The President submitted whether it would not be desirable to postpone observations on this subject until to-morrow, when other optical papers would be brought forward.—Sir David Brewster said there was one fact stated in the paper, on which, lest it should be forgotten, he would make a remark. He had, at the Bristol Meeting, explained a method of making even very small and imperfect crystals, of such substances as chromate of lead, useful in such researches as these; for although the fixed lines of the spectrum could not be seen in them distinctly, yet by interposing a thin plate of mica, their spectrum would be covered with a kind of network of parallel lines, by counting a given number of which the dispersive power could be had as accurately as by the aid of the fixed lines. As to the line G, Mr. Powell must be under some mistake, for the line G is a very well defined single line; but H and H′ are black bands, with many interposed lines; and perhaps these were not sufficiently distinguished, if the magnifying power of Mr. Powell's instrument were too small to define them; at all events, it would be well, in cases of this kind, if different observers in the same branch were acquainted with the powers and other particulars of the instruments used by each other. As to the absorption of the blue and violet rays, he had already pointed out a remedy for that inconvenience.

Mr. Dent then read a paper 'On the Construction of a portable Mercurial Pendulum, accompanied by Experiments.'

The mercurial pendulum having a glass cistern containing the mercury, is not well calculated for sending abroad. The use of glass is superseded, in the present case, by substituting a cistern made entirely of cast-iron, which not only possesses portability, but other advantages; for in this cistern the mercury can be boiled to expel the bubbles of air. Before adopting iron for the cistern, it was obviously necessary to ascertain, by repeated experiments, the effect of that metal on the enclosed mercury, as regarded variations in temperature, when compared with glass. In prosecuting these experiments, a remarkable effect of radiant heat was observed on these materials, worthy of being brought before the Association, as well as the description of the new mercurial pendulum, to the construction of which these experiments were subservient. The cistern is made entirely of cast-iron: the adoption of this metal admitted of the cistern being turned perfectly cylindrical within and without, and of thus simplifying the elements of calculation for the height of a perfect cylinder of mercury requisite for compensating the effects of variable temperature on the rod, an advantage which the use of glass did not allow. The homogeneity of the material also facilitates the reductions for temperature, by equalizing this throughout, and also admits of the bearings being diminished in number, and simplified in construction, when compared with the usual mercurial pendulum having glass cisterns. The suspending rod passes through a hollow screw, and is secured by a pin going through both. The hollow screw passes through the axis of the cistern, and the cistern is constructed to move round this screw, which admits of shortening or lengthening the pendulum for alteration in time. The edge of the cap belonging to the cistern is graduated, which subdivides the threads of the screw on the cistern being turned round for alteration in time. There is an aperture on the top of the jar, which allows of mercury being added or removed without unscrewing the cap of the cistern. This aperture is closed by a screw, which, as well as that on the cap, has a leathern collar to render the joints perfectly air-tight. Before any experiment on variable temperatures could be proceeded with, it was necessary that an oven (or hot chamber) should be constructed. A wooden case was made, having the door glazed with double glass panes, to admit of reading off the arc of vibration, also the thermometers. This wooden case was lined with a tin casing, which admitted of a current of hot water or air to circulate: the latter, after some experiments, was adopted. Great difficulty was presented by the thermometers at the top and lower part of the case not being of equal temperature—an inequality which would be fatal to the experiments. The obstacle was eventually overcome by introducing into the middle of the cross space below a hemisphere of tin, into the lining of the tin casing, just above the flame of the lamp, the heated air, passed from this reservoir up the two vertical spaces, which were filled with tin cuttings. The quantity and distribution of these cuttings were arranged, by trial, till a thermometer indicated the same temperature at top and bottom of the enclosed space. The top of the vertical sides of the tin casing were closed by that kind of ventilator, technically called a "hit-or-miss," by which a nice adjustment for the equality of temperature could be obtained. The object, in the first instance, was to determine, by experiment, the quantity of mercury to be put into the cistern, adapted to produce the necessary compensation for variation in the length of the rod. This quantity being found, by repeated experiments, to be greater than that which would have been given by calculation, the height being 7.9 inches, it became necessary to determine, by new experiments, what was the effect of the varying elasticity in the spring, by which the pendulum was suspended, produced by changes of temperature, and to separate this quantity from the effects of a variation in length, between the points of suspension and oscillation. To accomplish this, it was necessary that the pendulum should be preserved at a *constant* and *low* temperature, while the spring was exposed to a much higher one. For this purpose the pendulum was made to oscillate in a room in which the thermometer was at 52° Fah.; while a current of heated air was conveyed to the spring from a lamp placed at the other end of a tube. The spring had a circular perforation made in it opposite the end of the tube; and the bulb of a thermometer being placed opposite the aperture, the temperature of the spring was maintained at 95° for several hours. From this arrangement it was obvious, that if the pendulum was kept at the unvarying temperature of 52°, any change in its rate must be due to an alteration in the elasticity of the spring, and a small quantity due to its elongation, arising from the high temperature to which it was exposed; and the result of the experiment, as shown by a table, was a loss of 1.9 sec. in twenty-four hours. To render the inquiry complete, it was essential that the error arising from a change in temperature, in the *pendulum rod*, should be determined. A pendulum rod of steel was suspended, 0.293 in. in diameter; length, 3 ft. 7.1 in., from the spring to the point of attachment of the ball, which was suspended from the *end* of the rod: diameter of ball, 6.5 in., weight, 12.7 lb. The results shown by the table were, that the error due to the elongation of the rod alone, for 47° of Fah., is 12.0 secs. in twenty-four hours; while that of the varying elasticity and elongation is 1.9 sec. for 48° of Fah. in twenty-four hours. The author believes he was the first who separated the effects on the rates of chronometers arising from the elongation of the balance-spring, from those produced by variations in its elasticity; and in the present instance has completed the subject, by deducing the corresponding effects in the case of a pendulum. In order to try the comparative effects of changes of temperature on mercurial pendulums, with iron instead of glass cisterns, two thermometers were selected, whose scales throughout their range were consistent with each other; the glass and iron cisterns were filled with mercury; the thermometers immersed up to +18° Fah., care being taken that the bulbs were in the axis of the cisterns. They were then placed on a table, at two yards from the fire, and the author's attention was first attracted to an inequality in the temperature of the mercury contained in the two cisterns, indicated by the thermometers in them. The thermometers were changed, but still the same inequality appeared; that in the glass cistern always indicating a higher temperature by 5° than that in the iron one. To vary the experiment, a book was placed between the fire and each of the cisterns, when the inequality between the indications of the thermometers was reduced to 2°. The Argand lamp, used to observe with, was then removed, under an idea that this discrepancy would be still further reduced,—the room being left without other source of light or heat than the fire, after the lapse of three hours, the two thermometers indicated the same temperature nearly. On again exposing the two cisterns to the effect of direct radiant heat, the increased difference of temperature again appeared as before between the glass and iron cisterns. These experiments are recorded in a tabulated form, as also is an experiment under similar conditions, with only this difference, that the *surfaces* of both cisterns are *blackened* by a mixture of lamp-black and spirits of wine. Under this arrangement, although exposed to direct radiant heat, no discrepancy appeared between the thermometers placed in the glass or iron cisterns. These experiments, it should be borne in mind, were not undertaken with any view as to the effect of radiant heat, *but to determine the relative time of acquiring a certain increment and decrement of heat in the mercury contained in the cisterns* employed in the compensation pendulum, and the same as regards the *rod* of the pendulum. From the tables, which are very elaborate, the following results

are deduced:—Mercury acquires increment of heat from 26° to 75° Fah. in 2h. 15m., and the pendulum rod from 30° to 82° in 0h. 34m. Therefore the mercury contained in the cisterns takes upwards of *four times* the interval to arrive at the same temperature as the rod, under similar circumstances. The rod experiment was conducted by taking three small thermometers, and placing them on the rod, one in the centre, and the others at each end, making the contact by an amalgam. A table showing the effect of a strong and weak *suspension spring* was exhibited, giving the time for every decrease of 15m. of arc; from which experiment it was proved, that the length of time a free pendulum continues in vibration, is in the inverse ratio to the strength of the suspension spring of the pendulum; and that a pendulum suspended by too strong a spring will decrease in its arc to a greater extent than one supported by a weak spring, provided that any diminution of the maintaining force takes place.

Dr. Robinson was glad to see the iron cistern, for it rendered possible the boiling of the mercury in it, which he considered essential. However carefully the vessel was filled, there always remained minute air bubbles between its sides and the mercury, which were liable to changes of place, and could only be removed by this process. This was still more important when (as ought in all cases to be done) a correction depending on the state of the barometer was applied. Dr. R. had deduced this correction for his own clock, by the method of minimum squares, from actual observation; but it was far more easily done by swinging the pendulum in a vacuum apparatus. For the success of this, however, the total absence of air bubbles was requisite; and it had been found by a distinguished astronomer, (whose researches, though not yet published, he took the liberty of quoting,) that if this precaution were neglected, the *same* pendulum gave for a change of one inch in the barometer, a variation of rate from 0 to 1.5 seconds per day, the true value being 0.35. He would also profit by the opportunity, to suggest two other matters. The effect of high temperature on the pendulum consisted of two parts,—one its own expansion, the other that of the air in which it vibrated. The first only was corrected by the usual compensation; but the other varying according to a different law, required a separate compensation on a different principle, as the error resulting from its neglect was within the reach of observation. Another point was the neglect of the old knife-edge suspension, which he considered, in many respects, superior to the spring suspension, as indeed was clearly shown by Berthoud, and which he anxiously wished to see on trial among us.—Mr. Dent considered the formation of a knife edge difficult, and apprehended it would soon be blunted.—Dr. Robinson conceived that it would wear into a small cylinder, which would act as a roller, and still retain its good qualities.

Wednesday.

Prof. Whewell made a 'Report on the Discussions of Tides, performed under his direction, by means of the grant of money made for the purpose by the Association.'

Mr. Whewell began by remarking, that he had throughout adopted the method of curves, first systematically employed by Sir John Herschel, which consists in laying down a number of points expressing the results of individual observations, and then getting rid of the irregularities which these involve, by drawing, not a line *joining* the points, which would be a broken line, but by striking, with a bold but firm hand, a line *among* the points, so as to come as near as possible to the whole assemblage of them. In this manner the heights and lunitidal intervals were laid down as ordinates, and curves were drawn. This method of curves depends upon this fact, that the eye generalizes the relations of space more rapidly and surely than the intellect can generalize phenomena in any other way. We can hardly see any collection of detached points, but the eye, stimulated by the fancy, shapes them into figures; and if there be any trace of law or form in the position of the points, it may be thus detected. The curves of the tide observations being thus drawn, those of height, for example, exhibited a series of undulations having a summit and a depression for every fortnight, corresponding to the spring tides and neap tides. But these summits are alternately higher and lower. This alternation arises from the oval form of the moon's orbit, by means of which she is, in successive half revolutions, at a greater or less distance from the earth. The mean size of the undulation in the height curve, is intermediate between the greater and lesser ones; and if curve undulations of this magnitude be drawn, corresponding to the curve of observation, and if the difference of the two curves be taken, and again made the ordinate of a curve, this curve represents the *first residue* of the observations, —namely, that which remains when we have allowed for the semimenstrual inequality. This residue results mainly from the effect of the moon's parallax, (as has been just explained,) and also of her declination. A theoretical curve, depending upon this parallax and declination, may be drawn by proper adjustment of the parallax and declination correction, so as to correspond nearly with the first residue. This being done, the difference of these two curves, again laid down as the ordinate of a curve, is the *second residue*. And this second residue may, perhaps, again be reduced to rule, and a third residue obtained; and so on. The adjustment of the lunar parallax and declination corrections gives us, as we have said, the correction tables for those circumstances. Mr. Bunt's labours have been mainly directed to the determination of these corrections from the Bristol observations of 1834, 5, 6, 7, making each year's results distinct. The observations of each of these years gave a curve for the parallax correction; and these curves, drawn on the same axis, were laid before the Section, exhibiting the agreement of the different years. The same was done for declination. It appeared by such a comparison, that the observations of a single year gave very good tables of parallax and declination correction. This appeared, also, by remarking that the parallax curves for three minutes above and three minutes below the mean parallax, were exactly symmetrical, without any arbitrary improvement or smoothing whatever. It was stated, that the tables of corrections, obtained in this way from a single year's observations, are quite as good as those hitherto obtained from twenty-nine years. Mr. Whewell observed, that he expected to attain still further laws, from examination of the residue left by Mr. Bunt's observations. He had given the result of these discussions in his ninth series of Tide Researches, in the Philosophical Transactions. In that communication answers would be found to the thirteen following questions:—1. To which transit of the moon ought we to refer a tide? 2. How does a change of the epoch affect the semimenstrual inequalities? 3. How does a change of the epoch affect the (lunar) parallax correction of the time? 4. How does a change of the epoch affect the (lunar) declination correction of the time? 5. How does a change of the epoch affect the parallax correction of the heights? 6. How does a change of the epoch affect the declination correction of the heights? 7. Does the parallax correction of height vary as the parallax? 8. Does the parallax correction of time vary as the parallax? 9. Does the declination correction of heights vary as the square of the declination? 10. Does the declination correction of times vary as the square of the declination? 11. Can the laws of the corrections be deduced from a single year? 12. Are there any regular differences between the corrections of successive years? 13. Do the corrections at different places agree in laws and in amount?

Sir J. Herschel expressed his gratification at the results which were constantly developing themselves from the employment of curves for registering the phenomena, and, as it were, depicting them to the eye. These curves, among many other uses, had the advantage, as Mr. Whewell had just shown, of enabling us to compare most readily and simply theory with actual observation, thus detecting previously unsuspected discrepancies, and enabling us to find where error lurked in the theory, and to correct it. In fact, such was their utility, that most probably there would soon be no comparison instituted with isolated observations.

Mr. Russell, of Edinburgh, brought up the 'Report of the Committee (consisting of Sir John Robison and himself) on Waves.'

This report was a continuation of that of last year, published in the volume of the Transactions just issued. The following were the duties of the Committee:—1. To examine the phenomena of a certain kind of wave generated in a fluid, with the view of enabling us to understand the mechanism of its propagation, and so advance this department of hydrodynamical science. 2. To examine the nature of the connexion which exists between the generation of these waves in a fluid, and the resistance of the fluid to the motion of a floating body moved through it, as in the instance of a ship. And, 3. To investigate the nature of the connexion which exists between this wave, which Mr. Russell has called the "Great Wave of Translation," and the tidal wave, remarkable analogies having been already ascertained to exist between them; and to determine the effect of the wind on the propagation of the tide wave. In regard to the first department, the phenomena of the great wave of translation, the report of the preceding year contained a great portion of what had been done this year; but the observations were such that they would probably not be completed in several years. What they had done this year, had been to complete, as far as they possibly could, the second of these departments of investigation—that which related to the physical investigation of waves; and he was happy to think that all that could be done, had, he believed, been done, towards an understanding of the mechanism of the wave. He should, first of all, show what had been done in regard to the physical mechanism. They had previously determined the *formal* law of the wave; that was to say, they had determined that the velocity of the wave was totally independent of the manner in which it was generated, and depended only upon one circumstance—namely, the depth of the wave. But this was a merely formal result, and his present object was to investigate the physical constitution of the wave. In the formal investigation, they experienced considerable difficulty in ascertaining its form. The nature of a wave was such, that it was exceedingly difficult to detect its form, because it required an instantaneous observation, made with great precision, by a number of observers, in a small space, and with a minuteness which exceeded any means of ordinary observation. The method proposed by Prof. Stevelly had been satisfactorily applied, to determine the length of the wave, which they found little more than six times the depth of the fluid. But they also found that this did not hold in all cases; for when the wave was very high, the quantity was much greater; but when it was low, it was exactly the same. So that just when the wave was about to disappear, it was found that its length was six times the depth of the fluid. Now they could not define a form, unless by fitting it into some form already known; and from all that he could learn, the only class of curve to which there was any likelihood of fitting this wave, were the trochoidal curves, of which the cycloid was the limit. Now the form, as observed by him, (Mr. Russell,) was almost precisely cycloidal; but of double the length which the circumstance of its being the cycloid would have given, because it was manifest that taking the limits, and supposing the wave to be a cycloidal wave, it would only be the circumference of a circle, of which its height was the diameter, or little more than three times the depth of the fluid. But the true length was six times that quantity. Thus another set of curves became necessary. They found that the curve which the wave would fit was not a cycloid, but an analogous curve, which he (Mr. Russell) should be disposed to call the *semi-circular cycloid*. He should now state the results which they had come to. For the purpose of ascertaining the physical constitution of the wave, and how it was propagated, the reservoir which they had previously constructed was formed in a certain part of plate glass, the object being to float particles in water, and to observe the phenomena which took place among these particles while the wave was passing over them. He had previously examined many observations which had been made in Germany and elsewhere. Weber had described the oscillating wave and the ocean wave in all their phenomena; but neither the Webers nor any other investigator appeared to have recognized the existence of this great solitary wave of translation; they seemed to have limited their observations to the oscillatory and gregarious waves. He (Mr. Russell) called his the primary wave, or great wave of translation of the fluid.

The glass side of the vessel was carefully divided, so as to enable the eye to determine the results; and the following phenomena took place among the particles so invariably, that on the slightest observation he could calculate the results. Suppose that the particles were in a particular plane, at right angles to the direction of the motion of the wave, when first the wave came to that place, the particles would begin to move in the direction of that motion. They would move with accelerating velocity; they were at their maximum when the top of the wave was immediately over them; and from that moment the particles began to move forward with retarded velocity, and, at the instant when the wave left the place, they were at rest in precisely the same position to one another as they occupied previously to the translation. They were put forward without the slightest displacement. The next question was the path of translation, which was a curious and yet simple matter. While these particles were in their progress forward, they were also raised. They were transferred forward horizontally to a distance equal to twice the height of the wave; and the curves, which the uppermost particles described, were as exactly as possible semicircles, described on a radius equal to the height of the wave; and of the other particles, at greater depths, each of them described a semi-ellipse, whose major axis was equal to the diameter of the semicircle, and whose minor axis was to the radius of the semicircle in the same ratio as the height of the particle above the bottom to the whole depth of the fluid; the path of the lowest being a straight line. Considering, next, the vertical motion of the particles, it appeared that during the transit of the great wave, each particle was lifted upwards from a state of rest with an accelerated motion, left at its highest point for an instant at rest, from which it descended with a motion first of all accelerated, and then retarded, so as to be left perfectly at rest at its original height at the instant when the wave had passed away. Supposing, then, an elevation of fluid of this kind to have been once generated, the manner in which it propagated the wave might be adequately conceived to take place thus: any given series of particles, in the same vertical plane, would be more pressed forwards than a similar series behind them, under the anterior part of the elevation, and the difference of pressures would be the differential of the vertical ordinates in those planes; and this excess of pressure necessarily produced two effects, it forced the particles in one plane nearer to those in the other, and thus caused progression or horizontal translation of the particles; then, the same excess of pressure diminishing the distance between the particles, elevated the intervening column of fluid to a height inversely proportional to the distance of these planes from each other. The planes situated in the latter part of the elevation of fluid, were in a situation the reverse of this, and the difference of pressure permitted the particles to descend, and diminished the velocity of translation; and the sum of these increments and decrements being equal and in opposite directions, the particles were successively accelerated and elevated, retarded and depressed, by the same law. The curve which the wave described, was a very remarkable one. It was very nearly related to the cycloid, yet differed essentially from it. It was also related to the curve of sines. The ordinates of the cycloid consisted of the ordinates of a curve of sines, added to those of a circle. The ordinates of this wave-curve consisted of the ordinates of the curve of sines, added to those of a semicircle, whose diameter was double that of the circle. He, therefore, should designate this curve as the semicircular-cycloid. It was the only one which appeared to represent the observed phenomena. The next part of the subject to which he directed attention, was the relation which the translation wave bore to the phenomena of resistance of fluids. He had previously ascertained that the displacement of a fluid by a vessel took place, not in the body of the current, but solely by the generation of waves. Now, the manner in which they were generated, appeared to throw light upon the subject of the resistance of fluids; because they wished to have exactly the same transference for particles of matter which was required for transference of waves. They wished to remove the particles of fluid from a state of rest, and admit the vessel to pass through, and then allow them to return to their former places, just as in the wave the particles were first elevated above the surface, and then permitted to subside. Now they found that whenever the displacement took place, as in the wave, they had the phenomena of least resistance. So that, in forming a floating vessel with this wave-line disposed on alternate sides of the keel, so as to give such motion to the particles as to displace nothing more than was necessary, nor for a greater distance than was necessary to allow the vessel to pass, they obtained the solid of least resistance. Since that time, a variety of experiments on large vessels had been performed; steam vessels were now constructing on this form; and it was a remarkable fact, that the fastest vessel on the Thames was one to which this form had been given. Several other vessels had since been built on a large scale on this construction, and he was happy to find that a great number of ship-builders had adopted and were building vessels with great success on this line. It was scarcely credible, that a vessel should move at the rate of fifteen miles an hour, and not raise a spray—not raise anything like that high mass of water which was always found at the bows of vessels going at speed, but enter the water perfectly smooth, and leave it smooth, and as much at rest in the direction of the displacement as it was before the floating solid passed. This phenomenon had invariably accompanied all the vessels formed on this line. Now this appeared to him (Mr. Russell) to show the correctness of a remark made by D'Alembert, who, in his *Opuscules*, had given a demonstration, from the mathematical theory of fluids, that the resistance to a solid body, if properly formed, was nothing. He challenged the mathematicians of the day to disprove his assertion, which was never done; though what the proper form necessary for this purpose was, had not been assigned. Now he (Mr. Russell) thought he had quite manifested the possibility of a vessel moving through the water with little or no resistance. On making allowance for adhesion to the sides of the vessel, (which they knew might be done correctly, from experiments made by others,) they found that the resistance of the vessel was not one-twentieth part of the mere adhesion of the water to the sides of the vessel; so that the resistance from displacement of transference was nearly nothing. A large vessel having been made in this form, the following experiment was performed. Two oranges were placed in the direction in which the vessel moved; the person steering, after many attempts, at last succeeded in insinuating the prow of the vessel between the oranges; they rolled along the side of the vessel, remained in contact therewith, and returned at the wake, and when the vessel passed they remained at rest; they had been transferred horizontally, in the manner of a wave, and remained at rest in precisely the same position as they were when the transference commenced. This appeared to him to be the strongest test; and if this vessel was not a solid of least resistance, it was closely allied to it. [The Chairman: I should say it was a vessel of no resistance.] There was another thing which he might mention—namely, that as steam vessels built on this line did not produce the waves which were at present so injurious to the banks of rivers, &c., perhaps its introduction would be attended with great advantages in this respect. He felt certain, indeed, that this was a form to which ship-builders must ere long be driven. It was the theoretical form of least resistance, which he (Mr. Russell) gave at Dublin three years ago; but it was not until he discovered the law of transference of the wave, that he found he had hit upon the very form of displacement of the wave. Ship-builders had been in the habit of saying,—Whatever you do, let us have no hollow lines. The maxim now would be, at least of those ship-builders who had carefully examined the subject,—Let us have the hollow lines where we want them, and then we shall have plenty of scope for making fuller lines where they will not injure the progress of the vessel. He (Mr. Russell) should now have entered upon the connexion of the subject with the theory of tides, because he thought he had identified the theory of this wave with that of the tidal wave; and whatever influence the celestial mechanism might have upon the tides, they must yet depend upon terrestrial mechanism for bringing it to their doors: he thought they could get a great deal more knowledge about the theory of waves by tidal observations, because there they had a large, long, slow wave, which could be examined with great minuteness. Time, however, would not then permit him to enter upon the subject.

Sir W. R. Hamilton congratulated the Section on the increasing interest excited by these valuable researches of Mr. Russell on waves. It was now evident that they had a most important and direct relation to the doctrine of the tides; and he had no doubt, from the many imperfect glimpses which he was enabled to catch of their relation to the undulatory propagation of light, that they were in progress towards elucidating some of the mysteries of that mysterious physical process.—Mr. Whewell felt peculiar interest in that part of Mr. Russell's communication in which he described the internal motion of the molecules of the fluid during the progress of the wave, and traced each from its first disturbance of position to its return to rest after the passage of the wave; and he felt confident this was only the first step towards the solution of a most important problem in general hydrodynamics, by which we should at length be led to know the manner in which motions were propagated through fluids as perfectly as we at present know how forces communicate motions to cohering masses of matter. In describing the form of the curve of the wave Mr. Russell had proposed to call it the semicircular cycloid. He would be inclined to suggest the propriety of the term hemi-cycloid, in which both parts of the term are borrowed from the Greek, and the term semi-cycloid had already another acceptation.—The Rev. Prof. Chevallier asked, whether it was not necessary to vary the dimensions of the several curves of a vessel built on his principles to suit the velocity with which she was intended to move.—Mr. Russell answered that it was necessary.—Sir John Herschel was anxious that Mr. Russell should describe minutely to the Section the manner in which his experimental waves were generated; before, however, he gave Mr. Russell the opportunity of doing so by resuming his chair, he would state the reason why he was anxious on this point. Prof. Weber, as Mr. Russell had informed the Section, had some time since traced with much accuracy the motions of the several particles of the fluid mass during the transit of a wave, and had assigned the circular motion of each near the top, and the elliptic motions varying as you approached the bottom; but from his researches it would appear that each particle during the transit of a wave described the full circuit of its own curve, so as to return, as soon as the transit of the wave was completed, to the spot from which it had started when its equilibrium was first disturbed on the approach of the wave; whereas, if he understood Mr. Russell rightly, he had described cases in which the particles only described semicurves, beginning with the semicircle at the surface of the fluid; and did not describe the under-half of their curves, so as to return to the places from which they had started at the commencement of their motions. If he had apprehended this rightly, it furnished a case in the propagation of undulations in water very analogous in its physical circumstances to the loss of a half wave of light at the change from refraction to reflexion, and might perhaps lead to some conception of the origin of the distinction between positive and negative polarization in the luminous waves.—Mr. Russell said that he would describe the manner in which he generated the waves in his experimental canal, by which it would appear that he could at pleasure produce either a positive wave, or that which consisted of an elevated ridge only passing along the surface of the canal, or a negative wave consisting of a depression alone, similarly progressing, or a wave compounded of a positive and a negative wave, or one in which there was a ridge accompanied by a depression to constitute the wave. By penning up an elevated column of water at one end of his experimental canal, which was separated from the rest of the fluid by a sluice or diaphragm placed across it, when he suddenly opened this sluice the positive wave was generated; by depressing cautiously into the end of the canal a mass of foreign matter, as, for instance, a block of wood, a positive wave was also generated; by slowly and cautiously drawing out the same block the negative wave was generated; and by letting fall a considerable body of water from some height above

the surface at one end of the canal, a compound wave was generated. During the transit of a positive wave each molecule of the fluid only passed through the upper half of its semicurve, and thus only passed forward by the length of its longer axis without returning to its place; during the transit of a negative wave each molecule described only the lower portion of its curve, and thus went backward by the length of its axis, without returning again to the place from which it had been disturbed, while, during the transit of a compound wave, each molecule of the fluid described both the upper and under portion of its own curve, and thus returned at the conclusion of its transit to the place from which it had begun its motion when first disturbed.

The President then called on Sir David Brewster for two communications.

1. 'On some Preparations of the Eye,' by Mr. Clay Wallace, of New York.

Sir David Brewster laid before the Section a series of beautiful preparations of the eye, made by Mr. Clay Wallace, an able oculist in New York, calculated to establish some important points in the theory of vision. As no paper accompanied these preparations, which had reached him at Newcastle, Sir David Brewster explained to the meeting their general nature and importance. Mr. Clay Wallace, he stated, considers that he has discovered the apparatus by which the eye is adjusted to different distances. This adjustment is, he conceives, effected in two ways,—in eyes which have *spherical lenses* it is produced by a *falciform*, or hook-shaped muscle attached only to one side of the lens, which by its construction brings the crystalline lens nearer the retina. In this case, it is obvious that the lens will have a slight motion of rotation, and that the diameter, which was in the axis of vision previous to the contraction of the muscle, will be moved out of that axis after the adjustment, so that at different distances of the lens from the retina different diameters of it will be placed in the axis of vision. As the diameters of a sphere are all equal and similar, Mr. Clay Wallace considered that vision would be equally perfect along the different diameters of the lens, brought by rotation into the axis of vision. Sir David Brewster, however, remarked that he had never found among his numerous examinations of the lenses of fishes any which are perfectly spherical, as they were all either *oblate* or *prolate* spheroids, so that along the different diameters of the solid lens the vision would not be similarly performed. But, independent of this circumstance, he stated that in every solid lens there was only one line or axis in which vision could be perfectly distinct, namely, the axis of the optical figure, or series of *positive* and *negative* luminous sectors, which are seen by the analysis of polarized light. Along every other diameter the optical action of the lens is not symmetrical. When the lens is not a *sphere*, but *lenticular*, as in the human eye or in the eyes of most quadrupeds, Mr. Clay Wallace considers that the apparatus for adjustment is the ciliary processes, to which this office had been previously ascribed, though not on the same scientific grounds as those discovered by Mr. Wallace. One of the most important results of Mr. Wallace's dissections is the discovery of *fibres in the retina*. These fibres may be rendered distinctly visible. They diverge from the base of the optic nerve, and surround the *foramen ovale* of Soemmering at the extremity of the eye. Sir John Herschel had supposed such fibres to be requisite in the explanation of the theory of vision, and it is therefore doubly interesting to find that they have been actually discovered. Sir David Brewster concluded his observations by expressing a hope that anatomists in this country would turn their attention to this subject; and that with this view he would place the preparations of Mr. Clay Wallace in the Exhibition Rooms at Newcastle during the week.

2. 'On a new kind of Polarity in Homogeneous Light.'

At the last meeting of the Association (said Sir D. Brewster) I communicated to this Section an account of a *new property of light*, which did not admit of any explanation. Since that time, I have had occasion to repeat and vary the experiments; and, having found the same property exhibited in a series of analogous though different phenomena, I have no hesitation in considering this property of light as indicating a new species of *polarity* in the simple elements of light, whether *polarized* or *unpolarized*. In the original experiment, two pencils of perfectly homogeneous light, emanating from the same part of a well-formed spectrum, interfered after one of them had been retarded by transmission through a thin plate of glass. The fringes were exceedingly black, but no phenomena of colour were visible. I was anxious to observe what would take place when the retarded pencil passed through the edges of various plates differing very little in thickness, so that different parts of it suffered different degrees of retardation, for the preceding experiment entitled me to expect a series of overlapping bands and lines of different sizes. In making such an experiment, however, I encountered great difficulties, and I failed in every attempt to combine such a series of thin edges. I had recourse therefore to laminated crystals, and in an accidental cleavage of *sulphate* of lime I obtained the desired combination of edges. Upon looking through this plate at a perfect spectrum, in the manner described in my former communication, I was surprised to observe a splendid series of *bands* and *lines* crossing the whole spectrum, and shifting their place and changing their character by the slightest inclinations of the plate. But what surprised me most was to perceive that the spectrum exhibited the same phenomena as if it had been acted upon by absorbing media, so that we have here *dark lines* and the effects of local absorptions produced by the interference of an unretarded pencil with other pencils, proceeding in the same path with different degrees of retardation. The bearing of this unexpected result upon some of the most obscure questions in physical optics, I may have another opportunity of explaining. At present, I beg the attention of the meeting to another part of the experiment. We have seen that the effects of interference are distinctly developed in a certain position of the retarding plates. This position, when the effects are most distinct, is that in which the edges of the plates are turned *towards* the red end of the spectrum and are parallel to its fixed lines. If we give the plates a motion of rotation in their own plane, the bands and lines and the phenomena of absorption become less and less distinct as the angle between the edges of the plates and the lines of the spectrum increases. When this angle is 90° the bands disappear altogether, and during the next 90° of rotation they continue invisible. At 270° of azimuth they begin to re-appear, and attain their maximum distinctness at 360°, when they have returned to their original position. Here then we have certain phenomena of interference, and also of absorption, distinctly exhibited when the least refrangible side of the retarded ray is towards the *most* refrangible side of the spectrum, or towards the most refrangible side of the unretarded ray, while the same phenomena disappear altogether when the *most* refrangible side of the retarded ray is towards the *least* refrangible side of the unretarded ray; and between these two opposite positions we have phenomena of an intermediate character. Hence I conclude, that the different sides of the rays of homogeneous light have different properties when they are separated by prismatic refraction or by the diffraction of grooved surfaces or gratings,—that is, *these rays have polarity*. When light is rendered as homogeneous as possible by absorption, or when it is emitted in the most homogeneous state by certain coloured flames, it exhibits none of the indications of polarity above mentioned. The reason of this is, that the more or less refrangible sides of the rays lie in every direction, but as soon as these sides are arranged in the same direction by prismatic refraction or by diffraction, the light displays the same properties as if it had originally formed part of a spectrum.

Professor Whewell rose to ask Sir David Brewster whether the edge he had described as having been given to the crystals of sulphate of lime and of orpiment, did not in effect constitute an edge of several planes of different thickness; and wished to inquire from Sir D. B. whether the phenomenon depended in any way on the thickness of the retarding plate.—Sir D. Brewster replied, that the interval of the fringes appeared to increase as the thickness of the plate was diminished.—Prof. Lloyd requested to know (with reference to the possible explanation of the phenomenon), whether it was the same in the case of a spectrum cast upon a screen, as in that of a spectrum formed upon the focus of a telescope.—Sir D. Brewster replied, that he had not tried the experiment in that form, but had no doubt that the phenomena would be similar.—Prof. Chevallier begged to ask, whether there was any variation of the breadth of the bands he had described, as they were formed in light taken nearer to one end or the other of the solar spectrum.—Sir D. Brewster replied, that at the red end of the spectrum the bands were broad; those nearer to the violet end more narrow.—Sir John Herschel said, that in whatever point of view light was considered, or in whatever field of experiment respecting it we engaged, we were sure to meet with something to interest us by its novelty, or to astonish us by the unsuspected nature of the result. For his own part, he found that his long absence from home had placed him very much in arrear of the present state of the science; particularly as the indefatigable zeal and industry of Sir D. Brewster, and his very skilfully directed experimental researches, were constantly producing something of the deepest interest. We were, in these experiments, presented with what seemed to him inexplicable, on any supposition except the startling one of absorption by transparent media; and he saw very plainly, that this opened an entirely new field of optical discovery. With respect to the phenomenon of absorption, Sir J. H. remarked, that its explanation seemed to flow (at least in a general manner) from the dynamical theory of Cauchy. In that theory, the motions of ethereal molecules were represented by partial differential equations of the second order; and it was well known, that the integrals of such equations might be represented by a series of exponential terms, in which the exponents might be either real or imaginary. In the latter case, the formulæ were, of course, equivalent to a series of sines or cosines, and represented the periodical nature of the displacement. The other, he conceived, might represent the general phenomena of the degradation of light in its passage through absorbent media.—Sir D. Brewster replied, that he had found means, with the aid of glass partially decomposed, by having lain for some time in the earth, to exhibit all the various phenomena of absorption, diffraction, and also those of reflection; also, by a proper combination of such thin plates, he had been enabled to reproduce all the phenomena of absorption by nitrous acid gas; again, by interposing thin plates of mica, he had formerly described how, the fixed lines of the spectrum being removed, a multitude of parallel and equi-distant lines being introduced, we had the means of examining the dispersive power of very imperfect crystals, by simply counting the number of these lines contained between any two parts of the spectrum.—Sir W. R. Hamilton congratulated Sir David Brewster upon the rapid advance of his experimental researches. He could not, however, but regret, that he appeared to be getting so far in advance of theory; and yet he did not despair of seeing every one of those splendid results brought completely under the dominion of analysis.—Sir David replied, that he should be most happy indeed if Sir W. Hamilton's anticipations were verified. Candour, however, compelled him to say, that he saw no way whatever of accounting for these new properties of light by the undulatory theory. He would also take this opportunity of saying, that in expressing his opinion of Mr. Powell's communication on the theory of light, he did not consider the undulatory theory of dispersion as at all confirmed by any observations that had yet been made with bodies of high dispersive powers. Mr. Kelland had, as Mr. Powell mentioned, found, that a considerable difference between theory and experiment existed at the line G of the spectrum; and Mr. Powell endeavoured to remove the objection to the theory which this offered, by saying, that the line G is very indistinctly marked, and that, in the drawing of the spectrum given in Sir John Herschel's treatise on light, there was a very small line near G, which might be mistaken for it.—Sir D. Brewster remarked, that the line G was a remarkably distinct one, and easily recognized, as he knew from almost daily observation; and that, as Fraünhofer had obtained for this line the most accurate measures in various bodies, it would be practicable to obtain similar measures, with an apparatus equally effective. Sir David Brewster also stated, that he had communicated to Mr. Powell the fact, that in his (Mr.

P's) last paper, laid before the Ashmolean Society, the *two* lines, H H' had been, in some cases, regarded as *one*, whereas they were very remote, including 30 or 40 lines between them, so that any support of the undulatory theory derived from these observations was illusive. In reference to Mr. Powell's difficulties in finding a suitable crystal of *chromate of lead* for determining its dispersive power, Sir David stated, that the absorption of the *blue* and *green* rays of which Mr. Powell complained, could only be remedied by using small refracting angles, which he himself (Sir D. B.) had no difficulty in obtaining in his experiments; and he remarked, that the suggestion which he had made at the last meeting, to remove the difficulty of seeing the lines in *chromate of lead*, in consequence of imperfection of structure, or smallness of aperture, was the only one by which approximate results could be obtained. This method consisted in throwing over the *chromate of lead spectrum* a system of parallel lines produced by periodical action. Sir D. Brewster concluded his remarks by stating, that the explanation of dispersion given by the undulatory theory, could only be considered as confirmed, when it accorded with observations made with the same accuracy as those published, and with instruments of the same magnitude as those used by Fraünhofer.—Sir D. Brewster brought to the Section next day, his map of the lines of the spectrum, on a scale of about five feet, to establish his remarks respecting the lines G and H H'.

Sir William R. Hamilton then made a communication respecting the propagation of light *in vacuo*. The object of this communication was to advance the state of our knowledge respecting the law which regulates the attractions or repulsions of the particles of the ether on each other. The general differential equations of motion of any system of attracting or repelling points being reducible to the form—

$$\frac{d^2 x}{dt^2} = \text{S}.\ m,\ \Delta x\ f(r), \qquad (1)$$

the equations of minute vibration are of the form

$$\frac{d^2\ \delta x}{dt^2} = \text{S}.\ m,\ \big(\Delta\delta x.\ f(r)+\Delta x.\ \delta f(r)\big), \qquad (2)$$

in which

$$\delta f(r) = f'(r)\ \delta r, \qquad (3)$$

and

$$\delta r = \frac{\Delta x}{r}\Delta\delta x + \frac{\Delta y}{r}\Delta\delta y + \frac{\Delta z}{r}\Delta\delta z. \qquad (4)$$

A mode of satisfying the differential equations (2), and at the same time of representing a large class of the phenomena of light, is to assume,

$$\frac{\delta x}{\xi'} = \frac{\delta y}{\eta'} = \frac{\delta z}{\zeta'} = \text{const.} + \cos.\ \frac{2\pi(vt-ax-by-cz)}{\lambda}, \qquad (5)$$

in which ξ', η', ζ' are constants, depending on the extent and direction of vibration: a, b, c, are the cosines of the inclinations of the direction of propagation of a plane wave to the positive semi-axes of x, y, z; v is the velocity of propagation of that wave, and λ is the length of an undulation; and π is the semicircumference of a circle, of which the radius is unity. With this assumption (5), and with a natural and obvious supposition respecting a certain symmetry of arrangement in the ether, causing the sums of odd powers to vanish, it is permitted to substitute in (2) the expressions,—

$$\frac{d^2\ \delta x}{dt^2} = -\left(\frac{2\pi v}{\lambda}\right)^2 \delta x. \qquad (6)$$

$$\Delta\delta x = -\ \text{vers.}\ \Delta\theta.\ \delta x, \qquad (7)$$

in which $\Delta\theta = -\frac{2\pi}{\lambda}(a\Delta x + b\Delta y + c\Delta z)$; (8)

and thus arises a system of conditions of the form

$$\xi'\left(\frac{2\pi v}{\lambda}\right)^2 = \xi'\ m\ \text{S}.\left\{f(r)+\frac{\Delta x^2}{r}f'(r)\right\}\ \text{vers.}\ \Delta\theta$$
$$+\eta'\ m\ \text{S}.\ \frac{\Delta x\ \Delta y}{r}f'(r)\ \text{vers.}\ \Delta\theta$$
$$+\zeta'\ m\ \text{S}.\ \frac{\Delta x\ \Delta z}{r}f'(r)\ \text{vers.}\ \Delta\theta \qquad (9)$$

The masses m, of the etherial particles, being supposed each $=m$. Three conditions of this form (9) exist for every particle, and determine, in general, for any given values of a, b, c, λ, that is, for any given direction of propagation, and any given length of wave, the value of v, and the ratios of ξ', η', ζ', that is, the velocity of propagation of the wave, and the direction of vibration of the particle. Accordingly, with some slight differences of notation, they have been proposed for this purpose by Cauchy, and adopted by other mathematicians. Suppose now, for simplicity, that the plane wave is vertical, so that $c=o$; and let, at first, the direction of its propagation coincide with the positive semi-axis of x, so that b also vanishes, and a is $=1$. Then, for transversal vibrations, the expression for the square of the velocity of propagation is

$$v^2=\left(\frac{\lambda}{2\pi}\right)^2 m\ \text{S}\left\{f(r)+\frac{r^2-\Delta x^2}{2r}f'(r)\right\}\ \text{vers.}\ \frac{2\pi\Delta x}{\lambda}; \qquad (10)$$

which extends not only to the interplanetary spaces, but also to all ordinary transparent media, and contains, for them, the theoretical law of dispersion, which was first discovered by Cauchy, namely, the expression

$$v^2 = \text{A}_0 - \text{A}_1\,\lambda^{-2} + \text{A}_2\,\lambda^{-4}\ \&\text{c}. \qquad (11)$$

in which

$$\text{A}_i = \frac{(2\pi)^{2i}\ m}{1.2.3.4....(2i+2)}\text{S}\left\{f(r)+\frac{r^2-\Delta x_2}{2r}f'(r)\right\}\Delta x^{2i+2}. \qquad (12)$$

But, in order that this law may agree with the phenomena, it is essential that the series (11) should be convergent, even in its earliest terms; and this consideration enables us to exclude the supposition which has occurred to some mathematicians, that the particles of the ether attract each other with forces which are inversely as the squares of the distances between them. For if we suppose $rf(r)=r^{-2}$, and therefore $f(r)=r^{-3}$, $f'(r)=-3r^{-4}$, we shall have

$$\text{A}_i = \frac{1}{2}\,\frac{(2\pi)^{2i}\ m}{1.2.3.4...(2i+2)}\text{S}\left\{-r^{-3}+3r^{-5}\Delta x^2\right\}\Delta x^{2i+2}; \qquad (13)$$

and by extending the summation to particles, distant by several times the length of an undulation from the particle which they are supposed to attract, these sums (13) become extremely large, and the terms of the series (11) diverge very rapidly at first, though they always finish by converging. In fact, if we conceive a sphere, whose radius $=n\lambda=n$ times the length of an undulation (n being a large multiplier), and whose centre is at the attracted particle; and if we consider only the combined effect of the actions of all the particles within this sphere, we may, as a good approximation, convert each sum (13) into a triple definite integral, and thus obtain, for the general term of the series (11), the expression

$$(-1)^i\ \text{A}_i\ \lambda^{-2i} = \frac{(-1)^i\ 4\pi m n^2\ \lambda^2}{(2i+5)\varepsilon^3}\cdot\frac{(2\pi n)^{2i}}{1.2.3....(2i+3)}, \qquad (14)$$

ε being the mean interval between any two adjacent particles of the ether, so that the number of such particles contained in any sphere of radius r, is nearly $=\frac{4\pi r^3}{\varepsilon^3}$, if r be a large multiple of ε. And hence we find, by taking the sum of all these terms (14), the expression—

$$v^2 = \frac{\lambda^3}{\pi\varepsilon^3}\left\{\frac{1}{3}+\frac{\cos.\ 2\pi n}{(2\pi n)^2}-\frac{\sin.\ 2\pi n}{(2\pi n)^3}\right\}; \qquad (15)$$

so that, by taking the limit to which v^2 tends, when n is taken greater and greater, we get at last

$$v^2 = \frac{\lambda^2}{3\pi\varepsilon^3}, \qquad (16)$$

and

$$\frac{\lambda}{v} = \sqrt{3\pi\varepsilon^3}. \qquad (17)$$

But $\frac{\lambda}{v}$ expresses the time of oscillation of any one vibrating particle; this time would therefore be constant, if the particles attracted each other according to the law of the inverse square of the distance; and consequently this law is inadmissible, as being incompatible with the law of dispersion, and as allowing only light of some one colour to be propagated by the vibrations of any one system of particles. It had appeared to Sir William Hamilton important to reproduce these results, though he remarked that they agree substantially with those of Cauchy; because the law of the inverse square was one which naturally offered itself to the mind, and had, in fact, been proposed by at least one mathematician of high talent. There was, however, another law which had great claims on the attention of mathematicians, as having been proposed by Cauchy, to represent the phenomena of the propagation of the light *in vacuo*, namely, the law of a repulsive action, proportional inversely to the fourth power, or to the square of the square of the distance. M. Cauchy had, indeed, supposed that this law might hold good only for small distances; but in examining into its admissibility, it appeared fair to treat it as extending to all neighbouring particles which act on any one. But against this law also, Sir William Hamilton brought forward objections, which were founded partly on algebraical, and partly on numerical calculations, and which appeared to him decisive. In conclusion, he offered reasons for believing that the law of action of the particles of the ether on each other, resembles more the law which Poisson has in one of his memoirs proposed as likely to express the mutual action of the particles of ordinary and solid bodies, being perhaps of some such form as the following:—

$$rf(r) = -a.\ b^{-\left(\frac{r}{g\varepsilon}\right)^{h}} + a_{,}.\ b_{,}^{-\left(\frac{r}{g_{,}\varepsilon}\right)^{h_{,}}}; \qquad (18)$$

b and $b_{,}$ being each greater than unity, and $g, g_{,}, h, h_{,}$ being some large positive numbers, while a and $a_{,}$ are constant and positive multipliers, and ε is, as before, the mean or average interval between two adjacent particles. With such a law there would be a nearly constant repulsion, if a be greater than $a_{,}$, and if g be less than $g_{,}$, as long as $\frac{r}{g\varepsilon}$ is sensibly less than unity; but the force would rapidly change, as the distance r approached to $g\varepsilon$, and would then become a nearly constant attraction, until r became nearly $=g_{,}\varepsilon$; it would then diminish rapidly, and soon become insensible. Sir Wm. Hamilton did not, however, intend to exclude the hypothesis, that the function $rf(r)$ may contain several alternations of such repulsive and attractive terms,—much less did he deny that at great distances it may reduce itself to the law of the inverse square.

Prof. Lloyd begged to be allowed to offer a few remarks upon the general theory of Cauchy, and upon the law of repulsion of the ethereal molecules, deduced by that mathematician, and referred to by Sir W. Hamilton. In examining the theory of Cauchy, with reference to the propagation of light in uncrystallized media, he had found, that it followed, as a consequence of the views of that mathematician, that, in the expression for the square of the velocity of propagation, developed according to the inverse powers of the length of the wave, there should exist a certain theoretical relation among the co-efficients of the first three terms of the development. The truth of this relation, Prof. Lloyd stated, was not borne out by a comparison with the refractive indices of Fraünhofer for the fixed lines of the spectrum; and he had therefore come to the conclusion, that this portion of the theory of Cauchy (including his deduction of the law of repulsion according to the inverse fourth power of the distance) was invalid. He had no doubt that the invalidity arose, as Sir W. Hamilton had stated, from the assumption that sums might be converted into definite integrals. But he was also of opinion, that the complete solution of the problem of refraction could not be attained, until we took into consideration the action of the particles of the body, as well as those of the ether. He had attempted such an extension of the theory, in a paper read some time since to the Royal Irish Academy, of which an abstract has been printed in the proceedings of that Society. In the same paper, he had put forward some views with reference to the explanation of absorption, which he was glad to find, from the discussion of this morning, coincided with those of Sir John Herschel. It appeared to him, that the integral of the differential equations, which expresses the law of displacement of the ethereal molecules, should (in order to have its full generality) consist of a circular function, with an exponential co-efficient, from which it appeared that the amplitude of the displacement (and therefore the intensity of the light) must, in general, decrease with the distance traversed.

'Note on the Structure of the Vitreous Humour of the Eye of a Shark,' by Sir J. Herschel.

While crossing the Atlantic, on my return from the Cape, on the 31st of March of the present year, in lat. about 2° N., lon. about 20° W., we caught a shark. Having procured the eyes, which were very large, and extracted the crystalline lenses, the vitreous humour of each, in its capsule, presented the usual appearance of a very clear, transparent, gelatinous

mass, of little consistency, but yet forming, very distinctly, a connected and continuous body, easily separable from every other part. Wishing to examine it more narrowly, it was laid to drain on blotting-paper; and, as this grew saturated, more was applied, till it became apparent that the supply of watery liquid was much too great to be accounted for by adhering water or aqueous humour. Becoming curious to know to what extent the drainage might go, and expecting to find that, by carrying it to its limit, a gelatinous principle of much higher consistency might be insulated, I pierced it in various directions with a pointed instrument. At every thrust, a flow of liquid, somewhat ropy, but decidedly not gelatinous, emanated; and, by suspending it on a fork, and stabbing it in all directions with another, this liquid flowed so abundantly, as led me to conclude that the gelatinous appearance of this humour, in its natural state, is a mere illusion, and that, in fact, it consisted of a liquid no way gelatinous, inclosed in a structure of transparent, and, consequently, invisible cells. The vitreous humour of the other eye, insulated as far as possible, was therefore placed in a saucer, and beaten up with a fork, in the manner of an egg beaten up for culinary purposes. By this operation, the whole was resolved into a clear watery liquid, in which delicate membranous flocks could be perceived, and drawn out from the water in thready filaments, on the end of the fork. From this experiment, it is clear, that the vitreous humour (so called) of this fish is no jelly, but simply a clear liquid, inclosed in some close cellular structure of transparent membranous bags, which, by their obstruction to the free movements of the contained liquid, imitate the gelatinous state.

Sir D. Brewster observed, that he had frequently found the vitreous humours of fishes' eyes to exhibit the greatest variety of colour—green, rose-pink, &c.

Mr. Ball, of C. C. Cambridge, then read a paper 'On the meaning of the Arithmetical Symbols for Zero and Unity, when used in General Symbolical Algebra.'

It frequently happens, that in the operations of analysis, the arithmetical symbols for zero and unity are used in a manner quite independent of their ordinary signification, when used in the subordinate science. It becomes, therefore, a question of some importance, to ascertain what the more general meaning is which they ought then to receive. To give a few familiar examples, from the equation

$$\Delta^n x^m=(x+n)^m-n(x+n-1)^m+\frac{n(n-1)}{1.2}(x+n-2)^n,$$

&c.; by making $n=\frac{p}{q}$. $m=\frac{r}{s}$, $x=0$, ($\frac{p}{q}$ and $\frac{r}{s}$ being numerical fractions,) we have

$$\Delta^{\frac{p}{q}}0^{\frac{r}{s}}=\left(\frac{p}{q}\right)^{\frac{r}{s}}\frac{p}{-q}\left(\frac{p}{q}-1\right)^{\frac{r}{s}}, \&c.$$

Again, we know that log. $\frac{d}{dx}=1+\Delta$. In these and other similar cases, it is quite clear that the symbols 0 and 1 cannot be looked upon as mere arithmetical quantities. I shall now consider what the meaning is which ought to be attached to them, so as to agree with the general laws of combination of algebraic symbols, and with the particular meaning which they receive in arithmetical algebra. We find, that in arithmetic, the only mode by which we arrive at the symbol for zero (0) is, by the simultaneous addition and subtraction of the same numerical quantity. Hence we shall define the symbol (0) when used in general symbolical algebra, by the equation $0=a-a$, where a is any distributive function. Similarly, we find, that in arithmetic, unity, denoted by the symbol (1), arises from the division of one numerical quantity by another of the same value. Since we are exceedingly apt to confound the arithmetical with the symbolical meaning of unity, it will be convenient to denote it, when used in general analysis, by the Greek letter (Υ), defining it by the equation $\Upsilon=a^{\backprime}\,{}^{\prime}a$, where $a^{\backprime}$ and ${}^{\prime}a$ are relatively commutative. I shall notice some results of this mode of considering these quantities. From the equation $0=a-a$ we have $0^m=(a-a)^m=(\Upsilon-\Upsilon)^m a^m=a^m(\Upsilon-m+\frac{m(m-1)}{2}$, &c., where ($m$) may be any commutative and distributive function. When (m) is an integer, this equation takes the form $0^m\Sigma a_i-\Sigma a_i=0$, i. e. the arithmetical value of 0^m (when m is integer) $=0$; but this merely arises from the arithmetical value given to (m); but where (m) is any other distributive and commutative function, the series for 0^m does not terminate, and therefore is not necessarily 0. But even in the case where (m) is an integer, it does not follow that 0^m and 0 can be used one for the other, as this would indicate, that the expressions, where they enter, had been derived in the same manner. Thus, the equations $x-a=0$ and $x-a=0^{(2)}$ do not give the same relation between x and a; the first simply gives $x=a$, the second may arise from any one of the products $(x^{\frac{1}{2}}-a^{\frac{1}{2}})(n^{\frac{1}{2}}-a^{\frac{1}{2}})$, $(x^{\frac{1}{2}}-a^{\frac{1}{2}})(x^{\frac{1}{2}}-(+)^{\frac{1}{2}}a^{\frac{1}{2}})$, $(x^{\frac{1}{2}}-(+)^{\frac{1}{2}}a^{\frac{1}{2}})(n^{\frac{1}{2}}-(+)^{\frac{1}{2}}a^{\frac{1}{2}})$. From these results, we may derive the following brief sketch of a proof of the fundamental proposition of the theory of equations—viz. that every equation of the $(n)^{\text{th}}$ degree must have (n) roots of the form $(+)^{\frac{p}{q}}a$, where $\frac{p}{q}$ is a numerical fraction, and (a) a distributive and commutative function of the same class with the co-efficients of the equation. The form of the general equation of the $(n)^{\text{th}}$ degree, which has been generally considered by analysts, and to which the following proof applies, is $X=x^n+A_1x^{n-1};+A_rx^{n-r};+A_n=0^{(n)}$, where $0^{(n)}$ is an abbreviated expression for $0_1\,0_2\,..0_r\,0_n$ where $0_1=a_1-a_1$ and $0_r=a_r-a_r$. $0_n=a_n-a_n$. It is clear, that this is the correct mode of writing the equation, since it is always taken for granted that the right side may be divided (n) times by (n) different values of (0). Now, the meaning of the proposition $X=0^{(n)}=(a_1-a_1)..(a_n-a_n)$ is, that there are (n) quantities a_1, a_2, a_n such, that the product $(n-a_1)(n-a_2)..(n-a_n)$ will, when x is made equal to these (n) quantities in succession, coincide with the original equation. It does not follow, that these quantities are discoverable, but merely that the original proposition indicates their symbolical existence; the only *à priori* limitation to the values of x, and therefore to those of a_1, a_2, a_n is, to distributive and commutative functions. Comparing the co-efficients of the powers of x in the original equation and in the product $(n-a_1)..(n-a_n)$, we have (n) equations $A_1=+^{\frac{1}{2}}\Sigma_1$; $A_r=+^{\frac{r}{2}}\Sigma_r$; $A_n=+^{\frac{n}{2}}\Sigma_n$, denoting by $\Sigma_1, \Sigma_r, \Sigma_n$; the sums of the products of a_1, a_n taken 1 and 1, &c., r and r and n and n together. Hence we conclude, that the quantities $a_1, a_r..a_n$ must each be of the form $(+)^{\frac{p}{q}}a$, where $\left(\frac{p}{q}\right)$ is a numerical function, and (a) a distributive and commutative function of the same class, with the co-efficients A_1 and A_n; for if any one of them was of the form $+^dZ$, where (d) is not numerical, and (Z) not of the same class with A_1, A_n, we should have, from the equation $+A_1=+^{\frac{1}{2}}\Sigma_1$, an equation of the form $+^dZ=(+)^{\frac{p}{q}}A'$, which cannot be generally true: hence the general proposition, as before stated. We may easily derive from this the proposition, that (where the co-efficients are numerical) for every root of the form $(+)^{\frac{p}{q}}a$, there will be another of the form $(+)^{-\frac{p}{q}}a$, if (q) be greater than 2. From the foregoing statement, it is clear, that in this case, all the roots a_1 a_r a_n, are of the form $(+)^{\frac{r}{s}}a$, where a is a positive number. Suppose one root $a_1=(+)^{\frac{p}{q}}a$, where q is greater than 2, this cannot (generally) disappear, from the (n) equations $A_1=(+)^{\frac{1}{2}}\Sigma$ &c., unless some other root (a_2) be of the form $(+)^{\frac{m}{n}}a'$. By substituting in the above equations, we get $(+)^{\frac{p}{q}}a+(+)^{\frac{m}{n}}a'=+^{\frac{t}{2}}A'$, and $(+)^{\frac{p}{q}+\frac{m}{n}}aa'=(+)^{\frac{t}{2}}B'$; where t may $=0, 1, 2$, or any integer. The last equation requires that $\frac{m}{n}$ should $=-\frac{p}{q}$, and the first can only be satisfied by making $a=a'$; therefore $a_2=(+)^{-\frac{p}{q}}a$. To those who have read the interesting papers recently published by Mr. Gregory, of Trinity College, Cambridge, it will scarcely be necessary to state, that to him I am indebted for many of the views upon which this paper is founded; and to those who are inclined to undervalue the importance of those abstract questions which he has discussed, I may be allowed to observe, that the conclusions which are pointed out in the foregoing lines, even should they be deemed incorrect, show how very directly these fundamental questions may be brought to bear upon some of the most interesting points of mathematical science.

Sir W. Hamilton pointed out a different mode of generalizing the algebraic equation of the n^{th} degree, namely, $\Sigma_o^n A_i x^i=o$, by putting it under the form $\int_o^n A_i x^i di=o$, in which the function A_i may be either continuous or discontinuous.

Section B.—CHEMISTRY AND MINERALOGY.

Tuesday.

Mr. West read a paper 'On some Salts of Mercury, in which Chlorine and Cyanogen enter as component parts,' which had, however, unknown to Mr. West, been previously described.

The next was by Dr. T. Thomson, 'On Diabetic Sugar.'

This sugar has been considered as isomeric with starch sugar, though no accurate analysis of it has hitherto been made. Taste sweet; colour snow-white; melts at 239°; specific gravity after fusion, 1.56 at 65°; 100 parts of water dissolves 108 parts of this sugar. Boiling water dissolves any quantity whatever. Soluble in alcohol. It crystallizes, but so irregularly, that the shape of the crystals has not been ascertained. After being dried in vacuo over sulphuric acid, it loses an additional atom of water if it be exposed to the temperature of 212°, without losing weight. Its constituents were found by analysis, to be—

Carbon	37.23
Hydrogen	7.07
Oxygen	55.70
	100.

These proportions lead to the following formula:

12 atoms Carbon	= 9.	or per cent.	38.09
13 atoms Hydrogen	= 1.625	—	6 88
13 atoms Oxygen	= 13.	—	55.03
	23.625		100.

By Dr. Prout's analysis, starch sugar is $C^{12} H^{14} O^{14}$, or it contains an atom of water more than diabetes sugar. When diabetes sugar is digested with oxide of lead, an insoluble tasteless brown powder is obtained, composed of

Sugar	.577 or 20.17
Oxide of Lead	12.01 or 14 × 3 = 42
	17.78

It was obviously a compound of 1 atom of sugar with three atoms of oxide of lead. The sugar had lost 3 atoms of water, and had been converted into $C^{12} H^{10} O^{10}$, which is the formula for common sugar; thus the 3 atoms of water had been replaced by 3 atoms of oxide of lead.

'On a new case of Chemical action of Light, in the decoloration of recent solutions of Caustic Potass of commerce, and on the nature of the colouring matter,' by Robert Mallet.

The author stated that the caustic potass of commerce was well known to be a very impure compound, containing, besides potass, sulphate of potass, chlorides of potassium and iron, peroxide and carbonate of iron, silex, charcoal, and generally lime. He had also in one case found a trace of cobalt, from whatever source, and in several protoxide of lead, probably from the vessels used in its preparation. The colour of recent solutions of this potass in water freed from air by boiling, is apple green, and, occasionally, purplish green—which, whether exposed to air or not, or in dark or light, gradually disappears, leaving the solution colourless. A red precipitate of peroxide and carbonate of iron is produced on solution; but, after a time, the green solution, in losing colour, deposits a second in very small quantity, which Mr. Mallet has analyzed, and found to consist of

Sesquichloride iron........	15.7
Sesquioxide iron	83.2
	— = 98.9

This new compound, taking the equivalent of $Fe_2Cl_3 = 162.26$, and of $Fe_2O_3 = 80$, approaches closely to the composition $(Fe_2Cl_3) + 10\,(Fe_2O_3)$ or of ten atoms of sesquioxide of iron united to one of sesquichloride. All the iron contained in the green solution consists of *proto* salts. This high colour of solutions of common caustic potass is usually considered by authors as a case of mineral camelion—or to be produced by manganesiate of potass contained in the solution. The author, after the examination of a great number of specimens, could never find a trace of manganese. In this examination, in which he at times tried all the proposed methods, he gave the preference to that published by Dr. Liebig, of boiling with carbonate of lime or magnesia; he also found, where it could be applied, that oxide of zinc was a more delicate substitute for these bodies. He found that the various coloured rays of the spectrum transmitted through stained glasses, accelerated or retarded the decoloration of these solutions, and hence the formation of the compound $(Fe_2Cl_3) + 10\,(Fe_2O_3)$ in many various degrees; the green ray affecting it least, and more refrangible rays most. The results were given of experiments of exposure for periods of 200 hours of this solution, specific gravity 1082,—84, to sunshine under various coloured glass shades, and of observations made every two hours consecutively expressed by curves (on sheets ruled into squares), of which the ordinates expressed time, and the abscissæ, by coloured shadowing showed the changes of colour, passing through green, purple, red, brown, and white.

The periods of complete decoloration under the following coloured shades, were as the numbers annexed to them:—

Violet Glass, exposed to air	30 hours.
Ditto closed	50 —
Transparent Flint Glass, exposed to air	80 —
Flint, closed	115 —
Yellow	170 —
Blue	185 —
Orange	190 —
Red	200 —

Means were taken to insure an equality of temperature in all the bottles. The solution, in Bristol "bright metal" bottles, was found unchanged in colour in 200 hours, agreeing with Mrs. Somerville's observation of the power of this green glass to resist the blackening of chloride of silver. The effect here, as this lady also observed, of the violet ray, in promoting chemical change, is remarkable. This appears to be the first attempt made to establish, in a form susceptible of being applied to numbers, the varying chemical effects of the different parts of the spectrum. The effects of differing temperatures in all the bottles exposed was guarded against by coverings of ink of variable thickness, which Sir John Herschel had found transmitted white light undecomposed. The green colour of soapers' lees was explained by the foregoing, and the small number of observed cases of the chemical action of light (in so many of which chlorine played a part) pointed out, and the importance of further and more careful investigations of this branch of Chemico-Physics.

Mr. H. Pattinson 'On a new Process for the Extraction of Silver from Lead.'

The object of this paper was to lay before the Section an account of a discovery made by the author some time ago, the application of which to practice constitutes a new process in the arts, and forms an important improvement in the operation of extracting silver from lead. When the quantity of lead raised annually in England and Wales is taken into account, the importance of any improvement in the process of obtaining it with greater facility, will at once be duly appeciated. In 1828, the quantity raised was 45,500 tons, from the following sources:

	Tons.
Mines of Alston Moor, Weardale, Derwent, &c.....	22,000
Swalefield, Grassington-Palety, &c. Yorkshire......	4,700
Derbyshire	3,000
Shropshire	1,800
Devonshire and Cornwall	2,000
Wales, principally from Flintshire and Denbighshire	12,000
	45,500

The quantity has varied very little since the above date. The whole of this lead contains silver. Of the 22,000 tons of lead raised in the mines of Alston Moor, Weardale, Teesdale, &c., about 16,000 tons contain silver at the rate of from 6 to 12 oz. per ton, the average being about 5. The 4700 tons from Swaledale, &c. contain as nearly as possible 2 oz. per ton. The Derbyshire and Shropshire lead contains about an ounce, or the latter about 1½ oz. per ton. The lead from Devon and Cornwall contains from 20 oz. to 30 oz. per ton. One half of the lead from Flintshire and Denbigh averages from 4½ oz. to 6½ oz. per ton, and the other half 9 to 10 oz. The old process for separating this silver was by cupellation, or refining. This process depended on the well-known circumstance, that lead, at a red heat, is easily and readily converted into an oxide; while silver, almost at any temperature, retains its metallic state. It consists, therefore, in exposing lead to a full red heat, with free access of air, so that the whole of the lead may be converted into an oxide, and separated from the silver, which is left behind in a state of purity. This oxide being mixed with coal, and heated to full redness in a proper furnace, is reduced to the metallic state, called in commerce refined lead. But, it is impossible to carry on the process of refining, without a considerable loss of lead, as the oxide is very volatile, and flies off in large quantities from the refining surface in the form of a dense yellow smoke. The quantity of lead refined in 1828 would appear to be from

Alston Moor, Weardale, &c.	12,000
Devonshire and Cornwall	2,000
North Wales	4,000
	18,000

Upon which there would be lost in refining at least 1000 tons of lead. The importance of some more economical process was, therefore, at once apparent. The first idea which occurred to the author was, to distil the lead, and leave the silver behind. A quantity of lead was accordingly introduced into a stone-ware retort, and heated to redness for several hours. The retort was found to be quite in a soft state, from the intensity of the heat, and only a small portion of the lead had risen in the form of vapour, and been condensed on the upper part of its neck. But the fact was settled, that lead may be distilled, without determining whether it carried silver along with it. The next idea was, that as the specific gravity of silver is less than that of lead, there might possibly be a tendency in the silver to rise up to the top of a mass of lead, kept melted a long time at a uniform temperature; but, in no instance was there the least trace of any separation. Various other experiments were tried without success; but, in January 1829, the author happened to require lead in a state of powder, and, to obtain it, adopted the mode of stirring a portion of melted lead in a crucible, until it cooled below its point of fusion, by which the metal is obtained in the state of minute subdivision. In doing this, he was struck with the circumstance, that, as the lead is cooled down to nearly its fusing point, little particles of solid lead made their appearance like small crystals among the liquid lead, gradually increasing in quantity as the temperature fell. After observing this phenomenon once or twice, he began to conceive, that possibly some difference might be found in the proportions of silver, held by the part that crystallized, and the part that remained liquid. Accordingly, he divided a small quantity of lead into two portions, by melting it in a crucible, and allowing it to cool very slowly with constant stirring, until a considerable quantity crystallized, as already mentioned, from which the remainder, while still fluid, was poured off. An equal weight of each was then submitted to cupellation, when the button of silver from the liquid proved to be very much larger than that from the crystallized lead; and thus the somewhat curious fact was discovered, that fluid lead, holding silver in solution, suffers a portion to escape from it under certain circumstances in the act of becoming solid. The lead used in the original experiment was what is considered rich in silver. It contained 4 oz. 15 dwts. 8 grs. per ton, and was divided into a crystallized portion, found to contain 25 oz. 4 dwts. 21 grs.; and a fluid portion holding 79 oz. 11 dwts. 12 grs. per ton, the latter being necessarily much smaller than the former in quantity. It was not until the spring of the year 1833, that the author was conveniently circumstanced to proceed in applying to practice the principle which he had developed. Four or five tons of lead being melted in a large cast iron pot, was carefully freed by skimming from all dirt and oxide, and its surface made quite clean. It was then suffered to cool very slowly, care being taken to break off and mix with the fluid mass from time to time, any portion that might congeal on the sides of the pot. When the temperature had fallen sufficiently, small solid particles or crystals began to form, principally upon the surface of the melted mass. These, if suffered to remain, would have adhered together and formed a solid crust; but being continually struck, and the whole body of metal kept in motion by constant stirring, they sank down to the bottom of the pan, and soon appeared in considerable quantity. The author, however, did not succeed in making the lead sufficiently poor in silver; a pot filled, for example, with 8 oz. of lead, would yield at first crystals holding from 1 to 2 oz. of silver; in a little time, as the lead in the pot became richer, by receiving silver from the previously formed crystals, it yielded crystals of 2 to 3 oz., and the crystals became progressively richer, until in the end the original lead was divided into three parts of crystallizing lead; holding about 4 oz., and one part liquid lead holding about 20 oz. per ton. In order to drain the crystals more completely from the liquid lead, they were exposed after withdrawal from the pot to a reverberating flame, so as to melt out more liquid lead. In this way, from lead holding 12 oz. of silver per ton, four parts of lead containing ¾ oz. per ton, and one part containing 50 oz. per ton. The exposure, however, a second time to heat was expensive, and the author was induced in consequence to recommend in preference, the simple plan of repeated crystallization; it has now become the established process. The apparatus required for the separating process is exceedingly simple, and consists merely of a number of nearly hemispherical iron pots, each capable of holding about five tons of lead—the size for which, is about four feet diameter, and two feet three inches deep; one or two smaller pots, eighteen inches diameter by two feet deep; one required for the purpose of holding melted lead, in which the perforated iron ladles are to be occasionally dipped to keep them hot; and another pot about two feet ten inches diameter, by one foot ten inches deep, for melting the ultimate poor lead to be cast into pieces. These, with a few perforated iron ladles fifteen inches diameter, and five inches deep, and one or two whole ladles of lesser size, for casting the melted lead into pigs, are the principal articles required. The large pots are to be placed side by side in a line, each with a separate fire-place (upon which there must be an ashpit door as well as a fire door,) and also with a separate flue and damper, and the heat of the fire in some measure retained by shutting the ashpit door. Above the centre of this line of pots, at the height of six or eight feet, it is convenient to have a small iron railway with a frame or carriage on four wheels, to move backwards and forwards the whole length of the range of pots, from which is to depend a chain terminated by a hook at the bottom, and reaching to nearly the top of the pots. This is for the purpose of more easily conveying the ladles filled with crystals from pot to pot. All this being provided, one of the large pots is filled with lead containing silver, say 10 oz. per ton, and after it is melted and skimmed the fire is withdrawn, the damper put down, and the ashpit door closed, when it cools and crystallizes as already described. Crystals as they are formed are ladled out into the second pot, until about three quarters of the whole have been removed, which will contain about 5 oz. of silver per ton: upon this the operation is repeated, giving lead of 2 oz.; and by a third crystallization there is obtained from this, poor lead, holding not more than from 10 to 15 dwts. per ton, which is cast into pieces for sale as separated lead. The rich lead, on the other hand, is collected and repeatedly crystallized until it is made to contain 200 or 300 oz. per ton, after which the silver is extracted by cupellation. In working, the different pots at each stage are filled up always with lead of the same content of silver before beginning to crystallize, and a greater or less amount of crystals taken out as the operator may think fit; in which respect the practice differs almost at every establishment, but the process is so very simple, and the mode of proceeding so obvious, that it is unnecessary to give a more minute detail. By operating in the way described, it is evident that but a very small portion of lead is made to undergo the process of cupellation, not more than one-twentieth part, when 10 ounce lead is enriched to 200 ounce by repeated crystallization; and as the loss by

separation has not been found to exceed a 250th part of the whole lead, the loss by the joint processes becomes $\div \frac{1}{12}$ of $\frac{1}{20}+\frac{1}{250}$, or about one part in 120. The expense of separation is somewhat less than that of cupellation, so that by the reduction of expense and the reduction of the loss of lead, the extraction of silver is so far economized that 3 oz. per ton will now fully cover the charge. Applying this to the whole lead raised in the kingdom, as determined in 1828, we find that a much larger proportion can now be made to yield up its silver with advantage; we have for example, within the limits,

	Tons.
All the Lead of Alston Moor, Weardale, Teesdale, Derwent, &c.	22,000
Devonshire, Cornwall, and West Cumberland	2,000
Lead of North Wales	12,000

making a total of 36,000 tons, from which, if we deduct the quantity formerly refined, we have an increase of 18,000 tons; and allowing this to contain, which it will do on the lowest average, 6 oz. of silver per ton, and 3 oz. to cover the cost of extraction, we have a clear gain to the arts of 54,000 oz. of silver per annum. The annual saving of lead will be about 300 tons. It is an important fact connected with the separating process that the separated lead is much improved in quality, being more soft and ductile than ordinary lead. The reason of this is, that when the crystals are withdrawn from the liquid lead, being in the state of a coarse and very clean metallic powder, they are most readily acted on by the oxygen of the atmosphere, and come in contact with a great extent of heated surface, and the more oxidizable metals contained in the lead, as iron, zinc, &c. It is not improbable that the crystals are somewhat in the condition of spongy platinum, or Faraday's clean metallic plates. It now only remains to consider the reason why the lead, in the act of consolidation, does so to the exclusion of the silver; and the most simple explanation seems to be, that the process is an instance of true crystallization, the homogeneous particles of the lead coming together by their molecular attraction, and repelling the heterogeneous particles of the silver. It is true that, on account of the constant agitation, no trace of regular form can be seen in the solid mass, but if one of the pots in the act of consolidation be allowed to remain a few moments at rest, so that a skin forms on the surface, it will be found, on removing carefully a portion of this skin, that it is distinctly crystalline on its under surface; which proves that it is only the agitation which prevents its always exhibiting this structure. It is well known that when sea water freezes the ice is nearly fresh, the salt remaining dissolved in the surrounding water; and that salt water requires a lower temperature for freezing than pure water. To this fact the phenomena of the consolidation of lead containing silver appear to be analogous, the fusibility of the lead being somewhat increased by the alloy. It may seem anomalous that lead when alloyed with a metal, the melting point of which is so high, should be more easily fusible than the pure metal; but we have among metals many analogous circumstances. That such is the case is further proved by the fact, that when lead containing silver is gradually fused the first melted portions are richer in silver than the rest. The difference of fusibility is, however, not sufficient to allow of the separation of silver from lead by the ordinary process of eliquation; for in experiments made with the view of ascertaining how far this method might be employed in practice, in which the lead was exposed on the grating of the reverberatory furnace to a heat very cautiously increased till some drops of metal came out, it was found that, in lead containing 5 oz. 8 grs. of silver per ton, the first few drops sweated out contained 7 oz. 17 dwts. 9 grs. of silver; and that when two-thirds were drained off, the portion left contained still 3 oz. 13 dwts. 16 grs. of silver per ton. In another experiment, a piece of the same lead, drained very slowly till reduced to one-fourth or one-fifth of the original quantity, left lead containing per ton 1 oz. 17 dwts. 15 grs. of silver.

'Observations on some of the Products of the Action of Nitric Acid on Alcohol,' by Golding Bird, M.D.

In this paper the author wished chiefly to direct the attention of the Section to one or two substances produced during the action of nitric acid on alcohol, and which he believed to have been hitherto overlooked. Dr. Golding Bird's experiments were performed on the residue of the distillation performed in the pharmaceutical laboratory of Guy's Hospital. It has been stated by most chemists, that this residue left in the retort contained oxalic acid, and that this substance is a constant product of the process: this, however, appears from Dr. Bird's experiments to be erroneous; for on examining the fluid left in the retort, in which two gallons of alcohol and twenty-four ounces of nitric acid had been distilled until three pints were left, not a trace of oxalic acid was detected; and after a careful investigation, an organic acid was discovered, in considerable quantity, resulting from the decomposing action of nitric acid on the spirit employed. This, from the experiments detailed in the paper, appears to be identical with an acid long confounded with the malic, and termed by the French chemists Oxalhydric acid, (Hydroxalsäure, and Zuckersäure of German writers,) whose composition is represented by the formula, 3H, 4C, 6O. This acid is invariably produced during the preparation of nitrous ether by the assistance of heat,—no oxalic acid being found as long as *ether alone, or mixed with alcohol*, distils over; but as soon as this product is mixed with aldehyd, decomposition of the oxalhydric acid occurs, and oxalic acid is *then* produced. Dr. Bird also stated, that, providing the process is stopped at the point directed in the London Pharmacopœia, the distilled fluid is not aldehydiferous, which it is important to attend to, as the presence of a substance so pungent as aldehyd could not but be injurious in a fluid used so extensively in medicine for very opposite properties, as the spirits of nitrous ether. The production of aldehyd during the action of nitric acid on alcohol, has been noticed by Liebig in his excellent 'Handwörterbuch von reinem und angewandtem Chemie,' now publishing. The only circumstance which Dr. Bird deemed novel, was the fact of the appearance of oxalic acid in the residual fluid, and of aldehyd in the distilled matter, being nearly simultaneous. As a test for the presence of aldehyd in *spiritus etheris nitrici* of commerce, the author proposed the addition of a weak solution of potass, which would produce a yellow tint if this substance were present. From the formation of that substance, termed *aldehydharz* by Liebig, when hyponitrous ether is prepared by Dr. Black's process in the cold, acetic acid appears in abundance after the process has been carried on for a few days, which is not the case when heat is employed. All these different products—aldehyd, oxalhydric, acetic, and oxalic acids—may be considered as so many results of the oxygenating action of nitric acid on alcohol in different degrees, which Dr. Bird demonstrated by the aid of the following table:—

	Hydrogen.	Carbon.	Oxygen.
Alcohol	6	4	2
Aldehyd	4	4	2
Oxalhydric acid	3	4	6
Acetic acid	3	4	3
Oxalic acid (4 atoms)	—	4	8

From the numerous experiments detailed in the paper, the following were some of the inferences drawn by the author:—1. During the action of nitric acid on alcohol, no oxalic acid is formed as long as nitrous ether alone distils over. 2. That aldehyd is not produced, at least in any appreciable quantity, until oxalic acid appears in the retort, and the production of nitrous ether nearly ceases. 3. That during the preparation of nitrous ether *in the cold*, acetic acid is abundantly produced, and appears to replace the oxalhydric acid formed when heat is employed.—Dr. Bird also noticed, that in crystallizing the residual fluid in the retort, the first crop of oxalic acid crystals that appeared were of their ordinary form; but that those produced by subsequent evaporation were in pearly scales, much resembling in form the double cyanides described by Dr. R. H. Brett in the *Philosophical Magazine*,—some of which were exhibited by Mr. West to the Section this morning.

The next paper, also by Dr. Bird, was 'On the Possibility of obtaining, by Voltaic Action, Crystalline Metals, intermediate between the Poles or Electrodes.' These observations were in connexion with, and in support of, those laid before this Section last year, by the author, on the same subject.

After drawing the attention of the members to the important influence exercised by electric currents, and the probability of their affording some insight into the mysteries of the formation of mineral veins, Dr. Bird alluded to the results of the experiments detailed by him at Liverpool, and observed, that certain sources of fallacy existed, which he had not then been able to obviate, and which rendered the results less satisfactory; and after making a rapid sketch of the experiments of Fox and others, and pointing out their generally interesting, although unsatisfactory nature, he proceeded to describe a form of apparatus, arranged by Mr. Sandall, of St. Thomas's Hospital, by the aid of which he had obtained results which he considered as conclusive in favour of the possibility of the reduction of a metal intermediate between poles or electrodes. The apparatus consisted of a jar furnished with a *vertical* diaphragm of plaster of Paris, instead of an *horizontal* one, which Dr. Bird had previously used. In one cell of the jar was placed water, and in the other a solution of sulphate of copper. On immersing a compound arc, so that the zinc leg might dip into the water, and the copper leg into the metallic solution, an electric current of course ensued; and at the end of a month the solution of copper became nearly decolorated. On breaking up the apparatus, scarcely any crystals of copper were found on the negative electrode, whilst that surface of the plaster diaphragm bathed by the sulphate of copper, was covered with the reduced metal in a nodular or stalagmitic form. Specimens of the plaster covered with the copper were exhibited: they were exceedingly perfect, as far as metallic lustre and every required character were concerned; differing from those shown by Dr. Bird at Liverpool, in their want of crystalline surface. On breaking the mass of plaster crystals, little veins of copper were found disseminated through it in every direction, presenting a most marked resemblance to those met with on the large scale in nature.

Prof. Whewell observed, that the facts brought forward bore more directly upon the production of metallic veins, than any yet laid before the scientific world; and he stated, that, in his opinion, they were conclusive in favour of the important Faradian law—that the passage of an electric current was capable of effecting decompositions of compound bodies without the presence of poles or attracting surfaces.

Prof. Johnston described a compound of sulphate of lime, deposited from a high-pressure boiler, containing half an atom of water. The compound was in the form of a powder, and its composition was considered interesting, inasmuch as it contained a different quantity of water to any other composition of the kind. The compound of sulphates were better understood than any other, owing to their cheapness (sulphuric acid, for instance) and frequent use, but chemistry was not yet in possession of a tenth of the facts relating to even these. They were all aware that rock salt was procured in large quantities, and sulphate of lime was invariably found with it,—hence Berzelius threw out the suggestion that rock salt had been subjected to great heat after it had been deposited in the earth. He (Mr. Johnston) did not, however, think that this conclusion could be positively drawn from the facts before them, inasmuch as nature had means of forming substances, to a knowledge of which man had not, and perhaps never would attain.

Mr. R. Phillips 'On a Blue Pigment submitted to the Section last year by Dr. Traill.'

Mr. P. showed that the blue pigment in question was Prussian blue largely diluted, and rendered pale by ferrocyanide of antimony. Mr. Phillips made no observations on its properties as a pigment, which, we believe, was the object for which Dr. Traill had, at the last meeting, brought the compound before the Section.

WEDNESDAY.

'Note on the Constitution of Salts,' by Professor Graham.

The author hoped to be excused for drawing the attention of chemists to a distinction in saline combinations, which is, at present, too often overlooked, and confusion thereby occasioned. The orders of monobasic, bibasic, and tribasic salts, of which the phosphates proved types, have lately been greatly enlarged by the discoveries of Liebig and Dumas respecting vegetable acids, and the distinctive characters of these orders are well understood. The best

proof of an acid being bibasic or tribasic is, its combining at once with two bases which are isomorphous, or belong to the same natural family,—as phosphoric acid does with soda and ammonia in microcosmic salt, and tartaric acid with potash and soda in Rochelle salt. Water and magnesia, water and barytes, water and oxide of lead, are also constantly associated as bases in bibasic and tribasic salts, but never in true double salts, or combinations of two or more salts with each other, with which salts of the preceding orders are apt to be confounded. But it is too generally supposed that a metallic oxide cannot exist in a saline combination, except in the capacity of base, although in most of those bodies which are at present termed sub-salts, the whole or a portion of the metallic oxide is certainly not basic, but is attached to a really neutral salt, in a capacity similar to that of constitutional water, or water of crystallization. Oxide of copper, oxide of lead, barytes, and the other metallic oxides included in or related to the magnesian family, appear to rival water (which is a member of the same family) in the frequency with which they discharge this function in the constitution of saline compounds, particularly of those belonging to the organic kingdom. Thus, the neutral organic principle orcine combines with five atoms of oxide of lead, according to Dumas, which replace five atoms of water, which orcine otherwise possesses. But it should be brought prominently into view, that neither the water nor the oxide of lead is basic in these compounds, but superadded to the orcine, like constitutional water,—a distinction which is well expressed in their formulæ, by placing the symbols for water and oxide of lead after, and not before, that of orcine, or in the proper place for water of crystallization in the formula of a salt. Potash, soda, oxide of silver, and oxide of ammonium, on the other hand, are never found in this relation to a salt, or discharging any other function than that of base to an acid. Hence, there are no such compounds as subsalts of these bases. In Peligot's late admirable paper on the varieties of sugar ('Annales de Chimie,' &c., tom 67, p. 113), he has formed the compounds of that principle with barytes, lime, oxide of lead, and common salt, and determined their composition with great accuracy. Like preceding chemists, he considers them as salts, in which sugar is the acid, and the metallic oxide the base, and continues to speak of them as saccharates, although with an evident reserve. But the conclusion is by no means necessary, that sugar is an acid, and that the lime, oxide of lead, &c., are basic to it. On the contrary, sugar being a body neutral to test paper, is more likely to be a salt than an acid. That the metallic oxide attached to it discharges the function of the superadded water of crystallization of so many bodies, appears to me evident from the following circumstances:—1. It is separated from the sugar by the weakest acids, even by carbonic acid. 2. It replaces water in the sugar, which water can also be replaced in part by an equivalent proportion of chloride of sodium, or by the *hydrates* of barytes and lime. Now, basic water is never replaced by a salt, although constitutional water frequently is. 3. But the circumstance which is decisive of the lime and oxide of lead not being basic in the sugar compounds is, that analogous compounds do not exist containing potash, or any of the strong alkaline bases of its class. No acid is known which forms a salt with lime or lead, that does not also form a salt with potash or soda; but these last, as has been stated, are never present in any other capacity than that of bases, and are thus disqualified from replacing the water or magnesian oxide in combination with sugar. *The test of the non-basic character of water or a metallic oxide in a compound, is the absence of a parallel combination containing an oxide of the potash class.* The fact that the combined water in sugar is strongly attached, and cannot be removed by heat, is no proof that the water is basic, for many nitrates, hyposulphites, &c., are known, the constitutional or superadded water of which cannot be removed by the same agency without destroying the salts.

Constitution of Salts of—

Cyanuric acid ……	$Cy_3O_3 + 3\,MO$	{ Acid salt 2 HO+ KO. { Neutral .. HO+2 KO.
Fulminic acid ….	$Cy_2O_2 + 2\,MO$ ..	Salts 2 KO and KO+Aq., and $CriO + \dot{K}O$.
Cyanic acid………	$Cy\,O + MO$.	
Meconic acid….	$C_{14}\,H\,O_{11} + 3\,MO$ ..	Salts 3 $\dot{A}q$.; and $\dot{H} + 2\,\dot{A}q$.; and $2\,\dot{H}O + \dot{K}O$ and $\dot{H}O + 2\,\dot{K}O$.
Metameconic acid	$C_{12}\,H_2\,O_8 + 2\,MO$ ..	Salts 2 Aq.; and HO+KO and 2 $\dot{K}O$; also $\dot{H}O + \dot{A}q$.
Pyromeconic acid	$C_{10}\,H_3\,O_5 + MO$.	
Citric acid ……	$C_{12}\,H_5\,O_{11} + 3\,MO$ ..	Salts 3 AqO; 3 NaO dried at 200° centi.; 3 PbO; 2 PbO+HO.
Tartaric ………	$C_8\,H_4\,O_{10} + 2\,MO$ ..	Salts 2 $\dot{K}O$; HO+KO; KO+NaO.
Racemic	same.	Salts same.
Tannic acid ….	$C_{18}\,H_5\,O_9 + 3\,MO$ ..	Salts 3 PbO; 2 HO+PbO; and HO+2 PbO.
Gallic acid ….	$C_7\,H\,O_3 + 2\,MO$ ..	Salts 2 PbO; and HO+PbO.
Mucic ………	$C_{12}\,H_8\,O_{14} + 2\,MO$ ..	Salts 2 KO; HO+KO; 2 Aq.

(M represents an atom of metal.)

Dr. Gregory observed, that although the explanation presented by this table—which accounts for the apparent isomerism of cyanuric, fulminic, and cyanic acids—had been given by Liebig, the researches of Mr. Graham himself had led the way to the explanation.

'On the Influence of Voltaic Combination on Chemical Action,' by Dr. Andrews.

In dilute sulphuric acid, composed of one atom of the dry acid and eight atoms of water, the solution of distilled zinc is permanently accelerated, by connecting it with a plate of platina, immersed in the same liquid, so as to form a voltaic combination. In acid containing seven atoms of water, the ordinary action is at first increased, and afterwards rather diminished by contact with platina. But when zinc is heated in acid, containing less than this quantity of water, the connexion with platina transfers the evolution of gas, from the surface of the positive to that of the negative metal, and at the same time diminishes its quantity, and consequently retards the rate of solution of the zinc. The formation of a galvanic circle exerts, therefore, a reverse effect on the solution of zinc in sulphuric acid, containing more or less than seven atoms of water. The principal circumstances which influence these results are, the adhesion of the hydrogen gas to the surface of the zinc; the formation of sulphate of zinc, which is greatly facilitated by the presence of seven atoms of water in union with each atom of acid (that being the number of atoms of water of crystallization contained in it); and lastly, the proper action of the voltaic circle, which tends to diminish the solution of the zinc. In dilute acid, the first circumstance retards the action on the zinc alone, and the second facilitates its solution; then the platina surface enables the hydrogen to escape. But in the stronger acid, the voltaic association impedes the solution of the zinc, partly from the evolution of gas being transferred to the platina, and thus the saturated liquid being allowed to accumulate around the zinc plate; and partly from the real effect of the galvanic combination. That the proper tendency of a voltaic circle is, to diminish the chemical action of the solution on the electro-positive metal, the author endeavoured to show, from the consideration, that in ordinary solution the electricities thus developed have only an indefinitely small portion of liquid to traverse, while in voltaic solution their reunion can only be effected by passing across a column of variable extent, and composed of an imperfectly conducting substance. And, as the action is greater the nearer the plates are to each other, that action ought to attain a maximum when the distance between the plates vanishes, provided this condition could actually be realized.

Mr. Robert Mallet read his report of the experiments, instituted at the command and with the funds of the Association, 'On the Action of Sea and River Water, whether clear or foul, and at various temperatures, upon Iron, both cast and wrought,' by himself and Prof. E. Davy, of Dublin.

This subject is one of great interest to the engineer, as well as the chemist, as the former has no knowledge of the rate of action, relative or absolute, of water on iron under various conditions; and the chemist does not know precisely the changes which ensue during this action. The report is comprised under four principal sections, viz.:—First, a brief "*précis*" of the actual state of our chemical knowledge of the subject, *i. e.* of the reactions of air and water on iron. This embraces the following subdivisions of the subject: A. of the action of pure water free from air on iron, at various temperatures, and the nature of the compounds formed, which are at low temperatures, $(Fc_2O_3) + HO_2$ according to Berzelius, or $Fc\,O + Fc_2O_3$ according to Gay-Lussac. The latter is the oxide formed at a red heat according to Robiquet. B. Perfectly dry air has no action, or none below a red heat; the oxide then formed is $4\,Fe\,O + Fc_2O_3$; or, according to Mosander, $6\,Fe\,O + Fc_2O_3$. C. Air and water together,—viz. air combined with water, or moist air, act most rapidly on iron, producing when the vessel is shallow, $Fc^2O_3 + HO_2$, but if deep, Fc_2O_3 mixed with a large quantity of magnetic oxide. The second section of the report is devoted to a statement of the nature and extent of the experiments, which have been made on the great scale for the use of the engineer, as well as chemist, on the action of water on iron; for this purpose, boxes containing regularly formed specimens of nearly all the makes of iron in Britain, have been sunk, 1. in the clear water of Kingstown Harbour; 2. in the foul water of the same; 3. in foul river Liffey water; 4. in clear water of the same; and 5. in sea water maintained constantly at a temperature of 125° Fah. These will be examined twice a year for four years, and will give the relative and absolute rates of corrosion of the included specimens, which are contained in boxes so contrived as to allow free access of air and water to them. The third section contains a refutation of the method proposed by Mr. John B. Hartley, at Liverpool, for preserving iron by brass, which is shown by numerous and careful experiments, chiefly by Prof. Davy, and by appeal to the results of actual use, as well as the results of Schœnbein and other continental philosophers, to be wholly erroneous and contrary to all theory, and to be productive of a most rapidly increased corrosive action of water upon iron when present. Fourthly, a new method founded on electro-chemical agencies, is proposed for the protection of wrought and cast iron, now in progress of experiment; and, lastly, the report concludes with the statement of various desiderata upon this subject. All the details of the subject, such as the action of sea water in converting iron into a plumbaginous material, and the nature and properties, &c. of this subject, are necessarily omitted in a mere abstract.

'On the Construction of Apparatus for solidifying Carbonic Acid, and on the elastic force of Carbonic Acid Gas in contact with the liquid form of the Acid, at different Temperatures,' by Mr. Robert Addams.

Mr. Addams prefaced the communication by adverting to the original production of liquid carbonic acid by Dr. Faraday, in 1823, and also to the solidification of the acid by M. Thilorier, and then exhibited three kinds of instruments which he (Mr. Addams) had employed for the reduction of the gas into the liquid and solid forms. The first mode was mechanical, in which powerful hydraulic pumps were used to force gas from one vessel into a second, by filling the first with water, saline solutions, oil, or mercury; and in this apparatus a "*gauge of observation*" was attached, in order to see when the vessel was filled. The second kind of apparatus is a modification of that invented and used by Thilorier. The third includes the mechanical and the chemical methods, and by which, as stated, a saving of a large quantity of acid formed in the generator is effected; whereas by the arrangements of Thilorier's plan, two parts in three are suffered to rush into the atmosphere, and are lost. With this set of instruments are used two gauges of observation,—one to show when the generator is filled

with water by the pumps, and consequently all the free carbonic acid forced into the receiver; and the other to determine the quantity of liquid acid in the receiver. He likewise exhibited other instruments for drawing off and distilling liquid carbonic acid from one vessel into another, and mentioned some experiments which were in progress, and especially the action of potassium in liquid carbonic acid,—an action which indicated no decomposition of the real acid, but such as implied the presence of water, or a hydrous acid. A table of the elastic force or tension of the gas, over the liquid carbonic acid, was shown, for each ten degrees of the thermometer, beginning at zero, and terminating with 150 degrees. The following are some of the results:—

Degrees.	lb. per sq. inch.	Atmospheres of 15lb. each.
0	279.9	18.06
10	300.	20.
30	398.1	26.54
32	413.4	27.56
50	520.05	34.67
100	934.8	62.32
150	1495.65	99.71

Mr. Addams announced his intention of examining the pressure at higher temperatures, up to that of boiling water, and above; and asserted his belief that it may be profitably employed as an agent of motion—a substitute for steam,—not directly, as had been already tried by Mr. Brunel, but indirectly, and as a means to circulate or reciprocate other fluids. The solidification of the acid was shown, and the freezing of pounds of mercury in a few minutes, by the cooling influence which the solid acid exercises in passing again to the gaseous state.

SECTION C.—GEOLOGY AND GEOGRAPHY.
WEDNESDAY.

On taking the chair, Mr. Lyell said that his first duty was to inform the members, that there would be a meeting of the Section on Saturday at ten o'clock, so many papers having been delivered in, that it would be impossible otherwise to have them read and considered. On Friday there would be a geological excursion to Tynemouth and Whitby quarries, but the Section would be occupied with geographical business; and as the first paper to be read at this meeting related to antarctic discoveries he would request Sir George Back to take the chair.

Sir George Back, having taken the chair, said, that most of the papers on geographical subjects which had been read had chiefly referred to voyages in the northern regions; and though all had not been there accomplished that might be wished, yet enough had been done to attract the admiration and stimulate the attention of other nations to similar enterprises. He regretted, however, to say, that the same remark would not apply towards the South Pole; an immense space there remains unexplored, which he now briefly mentioned in order to introduce the following account:—

'On the Recent Expeditions to the Antarctic Seas,' by Captain Washington, R.N.

This paper was illustrated by a South Circumpolar Chart on a large scale, showing the tracks of all former navigators to these seas, from Dirk Gherritz, in 1599, to M. d'Urville, in 1838, including those of Tasman, in 1642; Cook, in 1773; Bellingshausen, in 1820; Weddell, in 1822; Biscoe, in 1831: and exhibiting a vast basin, nearly equal in extent to the Atlantic Ocean, unexplored by any ship, British or foreign. The writer pointed out that the ice in these regions was far from stationary; that Bellingshausen had sailed through a large space within the parallel of 60°, where Biscoe found ice that he could not penetrate. That where D'Urville had lately found barriers of field-ice, Weddell, in 1822, had advanced without difficulty to the latitude of 74½°, or within 16 degrees of the pole; and that it was evident from the accounts of all former navigators, that there was no physical obstacle to reaching a high south latitude, or, at any rate, of ascertaining those spots which theory pointed out as the positions where, with any degree of probability, the southern magnetic poles will be found. The paper also mentioned the expedition to the South Seas, which has just left this country, fitted out by several merchants, but chiefly under the direction of that spirited individual, Mr. Enderby, whose orders were to proceed in search of southern land, and to endeavour to attain as high a south latitude as practicable; and concluded with an earnest appeal to the British Association, that the glorious work of discovery begun by our distinguished countryman, Cook, might not be left incomplete; that all Europe looked to this country to solve the problem of Terrestrial Magnetism in the southern hemisphere; and that all civilized nations would unanimously point to that individual who has already planted the "red cross of England" on one of the northern magnetic poles, as the officer best fitted to be the leader of an expedition sent out for such a purpose. "Under a deep and abiding conviction," said the author, "that our country's future glory is identified with the encouragement of British enterprise, and that she would lose her high national character by ceding to another this opportunity of completing the work first traced out by Cook, I could not refrain from recording my sentiments, and conclude with the ardent hope that through the exertions of the British Association our wishes may be realized, and that ere long the southern cross may shine over an expedition sailing to the Polar seas; that cross sung by Dante and Camoens of old, which has served as a banner in a far more sacred cause; that cross, which, by its position, points out the hour of night to the Indian wandering over the pathless desert of Atacama, or the mariner ploughing the trackless ocean; that cross which brightly shone over Diaz, and Columbus, and Vasco de Gama; and that cross which I earnestly trust will once again shine over 'the meteor flag of England,' proudly waving over Antarctic land, discovered by the zeal and intrepidity of British seamen."

Sir George Back said, The account you have just heard, combines so much of the main points of the most striking events of past enterprises,—that little more seems left for me to say, than to acquiesce, which I most cordially do, in the concluding hope expressed by the writer. We have heard, from the testimony of former navigators, that there is no physical impossibility to prevent an approach towards the Southern Pole; and though M. d'Urville, with the *Astrolabe* and *Zélée*, has been compelled to return to the Bay of Conception, after a navigation of 52 days among ice; yet, had his crew been in good health, and had his means allowed him to have remained out longer, it is not improbable that some of those extraordinary movements of the ice—which he had himself so lately experienced—might have occurred, and have opened a passage to enable him to put in execution his cherished plan. That it was not so, might have been regretted, had we not an officer,—and why should he not mention his name?—Captain James Ross—who was both ready and willing, and in every way qualified to command in so patriotic an enterprise. He, too, would accomplish those great objects which science has in view, the establishing of the curves of magnetic dip, intensity, and variation; yet these would form but a part of the advantages which we might expect to derive from a voyage which would seem to be the birthright and the duty of a nation so essentially maritime as Great Britain. I have therefore (said the chairman), only to add my sincere wish that the expression of feeling manifested by this meeting may have its weight in recommending, in the proper quarter, the speedy equipment of an antarctic expedition.—Mr. Murchison said, that he gladly embraced the opportunity of expressing his cordial concurrence in the object of the paper which had just been read; and he felt certain, that if the British Association would recommend this subject with all the weight which from their station they were entitled to do, the great object of antarctic discovery would be no longer delayed.

Mr. Lyell then took the chair, and stated that two other excursions were in contemplation—on Monday to Marsden Rock, where the magnesian limestone was finely exposed, and the other to the lead mines of Mr. Beaumont, at Allenheads. The directors of the Newcastle and Carlisle Railway had offered a special train to convey the members to Haydon Bridge, and carriages would be there ready to take them on to Allenheads.

Mr. Murchison then gave an account of a geological map and sections of the border counties of England and Wales. The interesting rock formations which are laid down upon this map, and are fully described in his work, now on the eve of publication, have been separated by him from the upper transition series, under the name of the Silurian system. They are divided into four great groups, which he has called the Ludlow, Wenlock, Caradoc, and Llandeilo rocks. These were shown upon the map. He stated that the overlying strata were different from those in other parts of England, the magnesian limestone being wanting, and the carboniferous limestone containing no coal beds, as at Newcastle and other places. The most characteristic Silurian fossils were pointed out, and the interesting fact referred to as an important means of classifying these deposits, that none of the Silurian fossils were the same as those of the carboniferous system. Now, that his labours of seven years on this subject were completed, he intended to examine different tracts on the European continent, to ascertain whether similar deposits were there to be met with. He had lately obtained Silurian fossils from the Cape, through the kindness of Sir John Herschel, and he doubted not the Silurian group would yet be found an extensive one.

Professor Phillips observed, that Mr. Murchison's work would henceforth serve as a basis whereon to found investigations on the transition series. He mentioned, that within the last few days he had received a letter from Goldfuss, in which it is stated, that of forty species of encrinites, in the carboniferous system of Germany, not one is found in the Silurian system of England; and that the Silurian fossils are the same as those of the rocks called transition in Germany; and he noticed the importance of this fact, as showing the identity at remote points in the organic contents of the ancient sea. Professor Sedgwick remarked, that hitherto there had been a great break between the carboniferous and proper transition systems, which had now been filled up by the Silurian, a series of strata containing such numbers and kinds of fossils as formed for us an easy passage down into the old transition rocks—all the subdivisions which Mr. Murchison had established, and which answered so well for the Silurian, would not, perhaps, be found to answer for other places; but if so, its importance would be rather increased than lessened, as it would then supply the only link left of the great chain of events of which it was the monument.

Mr. Griffith then gave an account of his 'Geological Map of Ireland,' and of two remarkable sections in the south of that country.

Mr. Griffith traced out the boundaries of the various formations in Ireland, in a rapid manner. He then described at length two sections, one from Mount Leinster, in the county of Wexford, through the Monavullagh mountains to the sea shore in Cork; and the other in a north-west and south-east direction, through the southern part of the county Kerry, crossing the Mangerton mountains. Both of these exhibited red sandstone and conglomerate, resting horizontally on the highly inclined edges of slate rocks; the conglomerate being succeeded by a sandstone, containing fossil plants, and the hollows in the hills being often filled up by beds of carboniferous limestone. The conglomerate is often 800 feet thick, and constitutes the summits of some of the highest mountains; such, for example, as Carntoul, the highest in Ireland, which is 3,409 feet. In opposition to the views of Mr. Weaver, Mr. Griffith endeavoured to show that the limestone of the vale of the Lee, near Cork, was not transition limestone, but the true carboniferous. He hoped Professor Phillips and Mr. Murchison would be able to tell whether the fossils, which he exhibited from these sandstones, and from interstratified limestones, were Silurian or not. He strongly suspected they were so.

Professor Phillips and Mr. Murchison acknowledged themselves unable to decide from the specimens exhibited.—Dr. Buckland observed, that the same general direction was followed by all the great chains, not only of Great Britain and Ireland, but even by those in the north-west of Europe. This indicated the action of mighty forces along certain parallel lines: the evidence of their action being preserved in the position and contortions of the strata of sedimentary rocks, which had been raised by them. He had lately become acquainted with the structure of the country of Nassau, and was greatly struck with the perfect identity between it and the south of Ireland. He had not the smallest doubt

that the Silurian system was fully developed in Nassau. It seemed, also, from late geographical surveys undertaken by the American government, that the same formation existed in the Alleghanies. He commended, in the warmest manner, the liberality and intelligence both of the general and local governments of that country in organizing and carrying on effectively such surveys.—Captain Portlock pointed out a small tract of country in the county of Tyrone, on the borders of Derry, in which he had found the Silurian rocks distinctly developed. They rest on granitic rocks, and are covered by the old red sandstone; and contain the same genera of fossils as Mr. Murchison has described. These are quite distinct from those of the carboniferous system, though the latter is found within half a mile of the Silurian rocks.

A paper On the Stratification of Rocks,' by Mr. Liethart, of Newcastle, was next read.

After some preliminary remarks on stratification in general, and the insufficiency of all the explanations hitherto offered of it, the paper went on to detail the conditions and results of various experiments undertaken to show that galvanism may have been the agent which produced the determinate arrangement, which we call stratification. The alternate layers of different kinds of earthy matter, Mr. Liethart conceives very analogous to the plates of a galvanic pile; and, in his experiments, he introduced mixtures of different earthy matters into small glass tubes, to the ends of which the wires were then applied. He found, after some days, that traces of stratification were observable, and that in the central part of the tube the matter was much harder than towards the ends, where the traces were more strongly marked.

A short paper by Mr. Trimmer was read, 'On the occurrence of Marine Shells over the remains of Terrestrial Mammalia in Cefn Cave, in Denbighshire.'

The cave is in carboniferous limestone; the bones of the rhinoceros, hyæna, &c., are contained in marle beds and stalactite; and over these the fragments of various marine testaceæ, showing the irruption into this cave of a diluvial current. He referred to the Transactions of the Geological Society of Dublin, for a paper by himself on diluvium, in which the whole subject was discussed in relation to North Wales, and such deposits carefully distinguished from raised beaches.—Dr. Buckland referred to the importance of distinguishing between what were properly raised beaches, and deposits referable to diluvium. He thought nothing should be called a beach that was not between the level of high and low water. Neither did he see any reason why all the diluvium might not be regarded as of one age. The same great wave rushing over a country might form deposits of very different kinds in different places; and even in the same place substances so different might be found at different depths in the same deposit.—Mr. De la Beche said there were strong evidences in South Wales and Cornwall of northern currents. The stream tin of the latter county was undoubtedly a diluvial deposit carried along, as it were, by the tail of the wave. This stream tin, and the remains of man, were found among the roots of the trees of a submerged forest on the west coast of Cornwall; showing a recent change of level. He believed traces of a similar forest were found in many other parts; and was of opinion that it would be found generally round the coast of Britain.—Mr. Smith, of Jordanhill, made some remarks on the diluvium near Glasgow, and on the distinction between it and some beds of shells, to which he would call the attention of the Section on another day.

Section D.—Zoology and Botany.
Wednesday.

'On the Gemmiferous Bodies and Vermiform Filaments of Actiniæ,' by Mr. T. P. Teale.

The author stated, that as great differences of opinion existed amongst zoologists, as to the nature of the gemmiform bodies and vermiform appendages of Actiniæ, he had undertaken their investigation. Some general remarks on the structure of Actiniæ were premised, the author pointing out, by means of a large diagram, the various directions of the muscular septa, some lining the cavity and supporting the stomach of the animal, whilst others more delicate terminated in a mesentery, supporting the *gemmiferous bodies*, or what has been erroneously called the ovary. The division of the stomach into two lateral parts, giving to the whole animal a bilateral symmetry, was pointed out.

The *Gemmiferous Bodies* are about 200 in number, and appear as elongated masses attached along the inner border of most of the leaflets. Each is composed of several horizontal folds or plaits, which, when carefully unfolded, may, by the assistance of a lens, be seen to consist of two delicate layers of membrane, enveloping one closely compacted layer of gemmules. After enveloping the gemmules, the membranous layers become placed in opposition, and form the mesentery, by which the gemmiferous body is attached to the leaflet. The gemmules are round, except in an advanced stage of developement, when their outline becomes interrupted by the pressure of neighbouring gemmules. A well-marked central depression may also be seen indicating the situation of the oral aperture, but without tentacula; when of large size, they form considerable depressions in the gemmiform bodies, protruding before them their delicate investing membrane. In this state, they are readily detached by the point of a needle. Their size is nearly uniform, except a few small ones, scattered very generally amongst the whole. There is no gradation in size amongst them, as if they successively arrived at maturity, as imagined by Dr. Spix. Some of the gemmules are, however, less developed than others; and at the same season of the year, it is not uncommon to find individuals with the gemmules in very different stages of developement, and this is not limited to any particular season. The colour of the gemmules varies considerably. *The Vermiform Filaments.*—They are attached by a delicate mesentery to the internal border of each gemmiferous body; they are formed of numerous convolutions extending from the superior to the inferior part of the gemmiferous body. They are of a milk-white colour, about as thick as a horse-hair, extremely soft, yielding readily to pressure with a needle. Superiorly, the filaments are very minute, so that their origin cannot be detected. Inferiorly, they are of larger size and less convoluted, passing in a simple wavy line to the stomach, where they terminate. During life, these filaments exhibit a distinct vermicular motion, even after removal from the animal. On removing some from the animal, and placing them in sea-water, they exhibited considerable locomotive power, which lasted for some time, when their outline became obscured, and in twenty-four hours nothing remained but a whitish flocculent substance. This structure is best seen in its living state. In fresh water, it decomposes in half an hour, but in proof spirit less rapidly. The author has succeeded in preserving them best, by spreading the filament and its mesentery upon glass, upon which they may be dried. The function of these filaments is involved in obscurity. By many, they have been regarded as oviducts, but this the author thinks is very improbable, both from the minuteness of their terminations, the size of the gemmules, and the fact of ova never having been detected in them. In fact, the reproduction of Actiniæ must be looked upon as a strictly internal gemmiparous process, in which, the gemmules, when sufficiently matured, burst their envelope, and become lodged in the interseptal spaces, where they are exposed to the access and continued supplies of sea-water, the grand stimulus to their future developement. In the absence of any direct evidence as to the nature of the vermiform filaments, the author suspects that they are elongated follicular glands, analogous to the salivary, pancreatic, and hepatic follicles of animals a little higher in the scale of organization, supplying secretions subservient to the digestive process.

Mr. Gray observed, that it was extraordinary to find a man in the centre of our island, investigating the structure of objects on the coast, and giving to the world a knowledge of structure that had escaped the investigations of such men as Ehrenberg, Spix, &c.—The preparations in illustration of the paper were generally admired, and the Rev. F. W. Hope inquired what method Mr. Teale adopted.—Mr. Teale stated, that animals should not be placed in spirit before they were dead. He allowed them to become moribund, then divided them to prevent the secretions from hindering the action of the proof spirit into which he immersed them. This plan was applicable to all animals.

Mr. Wallace then read 'An Account of an Inosculation he had observed in two Trees,' thinking that the possibility had been doubted, from the nature of the observations on this subject at the meeting of last year.—Prof. Graham explained, that there was no difficulty in grafting trees of the same species or genus, and making them thrive on each other's sap; but that trees which were removed widely apart in structure, could not be engrafted, nor could perfect inosculation take place between their branches.

A drawing was exhibited, and the description given, of a new species of Ascaris, discovered by Dr. Bellingham, which he called *A. alata*. The distinctive character of this species was, that its posterior extremity was larger than its anterior.

The next paper was read by Capt. J. E. Cook, R.N., 'On the genera Pinus and Abies.'

The author commenced, by stating, that not less than seventy species of Pinus and Abies had been lately introduced into this country. The distribution of these throughout the world he divided into five groups:—1. Those of Old America, which included the United States, the Mississippi and Canada, with Labrador. 2. Those growing between the Pacific and Atlantic Oceans, in the district known by the name of the Rocky Mountains, and which might appropriately be called the "Douglas group." 3. The uplands of Mexico. 4. The Himalaya Mountains. 5. Europe. The first group contains about twenty species, none of which can be said to produce more than second-rate timber. They are fine trees in their native forests, but degenerate in Europe. 2. The "Douglas group"; of these there are about fifteen species, possessing all the qualifications for good timber, at the same time that they are evergreens, and grow quickly; and from the present condition of the young plants in England, the most sanguine anticipations of their successful culture in this country might be entertained. At present, however, little positive information had been obtained with regard to this group. 3. The species from Mexico are at present few in number; and too little information about them is possessed, to warrant any conjectures as to their worth. 4. Those in the Himalaya range are also few, and most are little known. Some of them may probably become naturalized in this country. The *Abies Webbiana* is a gigantic tree, but has not perfectly stood the last winter. *Abies Morinda* stood the winter very well. Both species are propagated by cuttings. 5. The European series are the most valuable. In this group the quality of the species is, as nearly as possible, in a direct ratio with the ability of the tree to resist cold; all the best species being found in an extreme northern latitude, or, in an equivalent situation on mountains in the south: no valuable species at all being found on the shores of the Mediterranean or the Baltic. The highest place given in the European series, is assigned to the *Pinus cembra*, and *P. uncinata*, both of which grow in their respective Alpine and Pyrenean forests, above the *P. sylvestris*, or Scotch fir, and both excel it in the quality of the timber. The *Pinus sylvestris* is next, and its range is from the Arctic circle to the Sierra da Guadarama, in Spain. The next tree in the series may be considered *P. Laricio*, which grows in the mountains of Corsica, at rather a high elevation, and in lat. 43°, and does not descend to the level of the Mediterranean. With this, both in latitude and elevation, is associated *P. Hispanica*, although in most respects it differs from other pines. Its range is from 39° to 43° N. lat., at the foot of the highest Pyrenees. Loudon copies the errors of French botanists, in stating this tree to exist in France, from whence it is separated by leagues of distance, and thousands of feet of elevation. These two species form about a middle zone in the European pinal vegetation, and their timber is found to occupy about a middle rank in quality, being superior to those below, and inferior to those above it, in its range. The next species is *Pinus pinaster*, whose northern habitat is the Sierra da Guadarama, and ranges immediately under *Pinus sylvestris*; it is not so good a tree as might be supposed from its range, as it grows in sultry valleys and situations unfavourable for the production of timber.

The *Pinus pinea* (Stone pine) has a timber nearly allied to the *P. pinaster*, its most northern habitat being in Old Castile, where it occurs in great quantity; and although it reaches a medium altitude, it is, like the last, found growing on sultry flats, as those of Andalusia, &c. The last of this series is the *Pinus Halepensis*, of which three varieties are known, and clothe the shores of the Mediterranean, on both sides, throughout its whole extent. The Abies do not admit of the same series of extended observation, being less in number and extent. The European species are certainly inferior to those of Pinus. The *A. excelsa* is the hardiest, and resists a damp soil probably better than *Pinus sylvestris*. The *A. pectinata* is found much further south, the last being found no further than Savoy, whilst this is found in the Pyrenees and Navarre, and a variety has been observed in Cephalonia; and no doubt great use could be made of it in our own culture. The larch, although in some respects an anomaly in the genus, follows the same rules. Its southern site is the highest Apennines in Piedmont, and its northward range is very great, but is never found at a low elevation. The *Pinus austriaca* probably belongs to this group, but the author knows little of it at present; as also *Pinus Satanica*, which grows in the Crimea. The cultivation of the hardier and more valuable species of these genera was strongly recommended from the results of the experiments of the Duke of Athol, who had found that timber of sufficiently good quality for the ordinary consumption of the Navy, might be grown at 1-140th the expense of oak, taking into consideration the rental of the land, and the ground occupied, besides the vast value given to the land by the fertilizing properties of the larch. The author considered that 100,000 acres of waste, taken from the Grampian hills, for the growth of larch, would, in two generations, not only supply all the ordinary wants of the country, but enable us to export it. In the west and south of England the *Pinus Laricio* and *P. Hispanica* would probably succeed best; the cedar of Lebanon might also be tried in these districts. He also recommended the larch to be cultivated by the proprietors of cold clay land in the north of England, as a means of improving the land by the deposition of its spiculæ, the trees being kept open for the admission of sheep for fifteen or twenty years, when the trees being gradually thinned, good open woodland would be formed, the soil of which would be good. No other species of tree should be mixed, as the larch is recommended merely as a fructifier or ameliorator of the soil.

Mr. Carr stated that *P. pinea* was a native of the south coast of France, and made several remarks on the growth of pines in this neighbourhood: he had found that in this district the *P. pinaster* grew too fast to produce good timber; in fact, it exhausted itself. The *P. pinea* grew better. He stated that the heart rot of these trees was not peculiar to England, as in France this was common, and the trees were afterwards used as pipes for irrigation.—Prof. Graham observed that *Pinus Morinda* bore this climate well. With regard to the *P. Douglasii*, there were two varieties,—one that had sustained little injury during the past hard winter, and another that had sustained very considerable injury. He might add, that the Araucarias had suffered no injury last winter in Edinburgh.—Sir Charles Monk observed that *P. Morinda* had been damaged during the past winter at his residence in Northumberland, fourteen miles from the town of Newcastle: he had specimens of Araucaria which were not at all injured during the past winter.—Prof. Graham thought that when trees grew but little, they were best enabled to stand the frost, but his had grown every year more and more, and yet stood last winter without injury. With respect to *P. Douglasii*, he had received a letter from Mr. Knight, just before his lamented death, in which that distinguished individual had informed him he had succeeded in grafting this pine upon the common larch. He had sent a fine specimen of *Halepensis* to the Horticultural Gardens at Chiswick, but it was destroyed one severe winter. He hasat present at his own residence a *P. Halepensis*, which had resisted the last winter. *Pinus Douglasii* was not sufficiently matured to produce seed, but it might be propagated by cuttings.—Mr. Carr has a fine specimen of *P. Douglasii* near the Cheviot Hills, but thinks it has the appearance of a tree out of climate, being covered with blebs.—Capt. Cook thought that the larch was not a good species on which to graft *P. Douglasii;* one nearer allied would be better.—Prof. Graham said, if timber was wanted, then it was best to graft on a closely allied species; but if fruit, then it was best to graft on the most distant.

Mr. Hope read a paper, entitled, 'Remarks on the modern Classification of Insects.'

The following is an outline of the communication:—1st, That modern entomologists, in their arrangements, have attended almost entirely to external organization. 2ndly, Internal organization has only been partially attended to: the alimentary canal, on which much stress is placed, cannot be considered as a criterion of an animal or a vegetable feeder, and is ill-adapted for the classification of insects. 3rdly, No uniform principle of arrangement has been entirely carried out: all have been interfered with by the introduction of other principles of a secondary and minor importance. 4thly, It is only from increased attention to the *Nervous System* that we can expect a more natural system than what exists at present.—Tables of genera illustrated some of the above points.

Mr. Gray objected to the nervous system or any other internal organ being used as a means of classification; the external forms were much more easily discovered, and external conformation so often accompanied internal variations, that it was unnecessary to have recourse to minute dissections to discover the position of any object in classification. He supported his opinions by numerous facts which he pointed out in the modern system of classification.—Mr. Hope stated, that external form was so fallacious a guide, that all naturalists were abandoning it. The nervous system was the fundamental point in the system of animals on which all other parts were dependent: it was the point on which all the great divisions of the animal kingdom were founded; it constituted the surest means of distinction, and every day added to our knowledge of its varied forms. In the class Insecta, great progress was making, and by the labours of such men as Müller, Ehrenberg, Grant, and Newport, the latter of whom he might call the Lyonnet of England, they were making rapid strides towards establishing a classification of insects on a knowledge of their nervous structure.

Mr. G. B. Sowerby exhibited specimens of *Lycopodium lepidophyllum* of Hooker, pointing out the facts, that, instead of having numerous stems disposed in a stellated form, as stated by Hooker, the whole plant consists of a single spiral branched stem: and that the fructifying spikes, instead of being "*acutè triquetris*," are of a *square form*, with sharp edges. Mr. Sowerby (not being himself a botanist) wished to know from the Section, whether these differences were to be regarded as specific, or as a variation of form.—Dr. Greville stated, he believed the description in Hooker's 'Icones Plantarum' was incorrect.

Mr. G. B. Sowerby then laid before the Section some specimens of *Encrinus moniliformis*, displaying various monstrosities of form in the number of plates of the pelvis, costals and scapulars, as well as in the arms, in which were manifest several variations from the normal form of the species, from five to six pelvic plates, costals, and scapulars, and from nine to thirteen arms: he directed the attention of the Section to the great variation in the form of the tubercles on the external surface of the joints of the fingers, some of the joints being almost destitute of tubercles, others having them very acute and regular, and others again being extremely irregular: he also pointed out the fact, that when two vertebral columns had been pressed against each other, both had corresponding depressions, indicating, in his opinion, that Miller was correct in his supposition, that these animals were *soft* when living.

A paper was read by Mr. Arthur Strickland, 'On the Ardea Alba.'

Mr. Strickland stated, that this bird had been unjustly excluded from the catalogue of occasional visitors to this country by late authors, as he could prove it had been killed of late years in more cases than one, on unquestionable authority. The first instance was twelve or thirteen years ago: a bird of this species was seen for some weeks about Hornsea Moor in the East Riding of Yorkshire; it was after that time procured by a friend of his, who had it preserved, and after keeping it some years, presented it to him, in whose collection it is at present in perfect preservation; another, in full summer plumage, was killed at Scarborough, near Beverley, about three years ago, and is now in the possession of Mr. James Hall, of that place. Another specimen of this bird is in the collection of Mr. Foljambe, of Osberton, with a label on the case, stating it to have been killed near that place. A careful examination of these specimens will, he has no doubt, prove that this bird is properly separated from the large egret of North America, a bird that has been frequently placed in our collections for the British species.

Mr. Strickland afterwards read a description of a large fish of the shark kind, that had been lately caught on the east coast of Yorkshire, possessing a smooth and spinous skin, like some of the Ray tribe, and several other characters very different from any fish yet described as having been taken on the British shores.

Mr. Fox was glad to find that the *Ardea alba* was really a British visitor: it had been described as such more than fifty years, but rejected in recent catalogues. He inquired, if any ornithologist had seen *Ardea candidissima* in this country; he believed it was very common to find this species under the name of the little egret (*A. egretta*) in the museums of this country. He believed they were generally foreign specimens.—Mr. Selby had never heard of *A. candidissima* being shot in this country.—Mr. Yarrell thought the fish would be found to resemble one brought from Africa by Dr. Smith; it certainly was different from any he had ever seen or heard of as caught on this coast. This fish he thought belonged to the genus *Scyllium*, of Cuvier; but, Dr. Smith had found it necessary to subdivide that genus, and this animal might be referred to the group thus separated from the species originally placed in the genus.—Mr. Forbes stated, that he had lately taken off the Isle of Man two specimens of the lancelot.

Section E.—MEDICAL SCIENCE.—Wednesday.

Dr. Yelloly (the Chairman) laid before the Section, a model of an improved acoustic instrument, for the purpose of assisting in cases of partial deafness. He alluded to the very defective nature of our present instruments, both as to utility and convenience, and the importance of having some experimental researches made on the subject; and he proposed, that the Section recommend to the General Committee the appointment of a committee to investigate the subject experimentally, and to make a report on the subject, at the next meeting of the Association. Dr. Granville seconded the motion, and proposed that Dr. Yelloly and Dr. Arnott should be named as the Committee.

Professor Owen then read a paper 'On the Structure of Teeth, and the resemblance of Ivory to Bone, as illustrated by microscopical examination of the teeth of man, and of various existing and extinct animals.'

Mr. Owen commenced by showing, that he had availed himself of the advantage afforded by the British Association—viz., that in the communications brought before a sectional committee, a fuller and more detailed retrospect of the progressive steps which have led to any remarkable discovery, is not only permissible, but peculiarly congenial to its general views and objects; and he therefore entered into a full detail of the recent investigations, especially those of Purkinje, Müller, and Retzius, on the intimate structure of the teeth, and particularly dwelt on the discoveries of the latter author, as regarded the structure of the human tooth. After describing the mode of arrangement of the particles of the earthy salts, which characterizes true bone, Professor Owen proceeded to state, that until a very recent period the analogy of tooth to bone was supposed to extend no farther than related to the chemical composition of of the hardening material, while the arrangement of this earthy constituent, as well as its mode of deposition during the growth of the entire tooth, were considered to be wholly different from that of bone, and to agree with the mode of growth of hair, and other so-called extra-vascular parts, with which the teeth undoubtedly closely correspond in the general vital properties. He observed, that the supposed proofs of the lami-

nated structure of teeth, derived from the appearances presented by the teeth of growing animals, fed alternately with madder and ordinary food, and by those which often occur during the progress of decomposition of certain teeth, which are then resolved into a series of concentric or superimposed laminæ, were equally applicable to true bone, and were quite unavailable in illustrating the point under consideration: and that the appearances presented by the superficies of vertical sections of teeth, viewed with the naked eye or a low magnifying power, were due, not to the intervals of separate and superimposed lamellæ, but to the different refractions of light, caused by the undulation of a series of parallel tubes proceeding in a contrary direction to the supposed lamellæ. This apparent lamellated structure, however, is not constant, nor equally plain in different teeth; on the contrary, the fractured surface, or the polished section of the human and many other teeth, presents a silky or iridescent lustre, which has attracted the attention of several anatomists. Professor Owen observed, that Malpighi, in whose works may be detected the germs of almost all the great anatomical truths, which have subsequently been matured and established, conceived that the teeth were composed of minute fibres reticularly interwoven; and Leewenhoek in 1683, had discovered, that the apparent fibres of tooth were, in reality, minute tubes. The tubular structure of ivory was rediscovered by Purkinje, in 1835, who also added several interesting facts regarding the structure of the enamel, and more especially determined the nature of that distinct layer of substance, which surrounds the fang in the simple teeth of man and carnivorous animals. The interesting experiments of Professor Müller, on the nature and contents of the *dental tubuli* were then noticed; and, lastly, a condensed analysis was given of the laborious and accurate microscopical observations of Professor Retzius, as related in the original Swedish memoir of that author on the structure of the teeth. Besides confirming the fact, that the ivory or bony constituent of a tooth consists of the fundamental *zahn-substanze* of Purkinje, minute tubes lodged in a transparent medium, disposed in a radiated arrangement in lines, proceeding in a direction perpendicular to the superficies of the tooth, Professor Retzius has more particularly observed, and described the dichotomous branching of the primary tubes; the minuter ramuli sent off throughout the course of the main tubes into the clear interspaces; the calcigerous cells with which those fine branches communicate, and the terminal ramifications of the tubuli; their anastomoses with each other, and with calcigerous cells at the superficies of the ivory, or bony part of the tooth. Professor Owen also discussed the opinion advanced by Professor Retzius, as to the function of this elaborate contexture of branched and anastomosing tubes and cells, in conveying, by capillary attraction, a slow current of nutritive or preservative fluid, through the entire substance of the tooth; which fluid might be derived either from the superficies of the pulp in the internal cavity of the tooth, or from the corpuscles or cells of the external layer of cortical substance or *cæmentum*,—with the tubes radiating from which corpuscles, the fine terminal tubes of the ivory anastomose. Professor Owen concluded the critical portion of his communication, by explaining the views entertained by Professor Retzius on the analogy subsisting between tooth and bone, which analogy he then proceeded to illustrate by his own observations on the structure of recent and fossil teeth.

Of the descriptions given by Prof. Owen of these teeth we select the following examples:—In the *Orang Utan* the main calcigerous tubes of the ivory, which radiate from the central cavity of the pulp are somewhat larger than those in man; they present the same primary curvatures, but less numerous and less strongly marked secondary curvatures.* In the crown of the tooth of the orang, the dental tubes are chiefly branched at their extremities, while towards the apex of the fang the main tubes are surrounded by exceedingly fine and close-set branches, which subdivide in their course. The nearer the crown the larger are these branches; they are curved, with the concavity towards the pulp. In the *Leopard*, the dental tubuli of the canine teeth are chiefly remarkable for the number of their ramifications, and the beautiful curvatures of the same. In the *Mole*, the main tubes are remarkable for their width and shortness; they are as large at their commencement as in the human tooth, but soon divide at their extremities into a number of smaller branches, which again subdivide; the terminal twigs anastomosing and communicating with minute calcigerous cells immediately beneath the enamel. The *Sloth* presents, as Retzius rightly observes, the greatest deviation from the ordinary structure of the teeth of mammalia. The substance, which corresponds to the true ivory, forms only a very thin layer on the superficies of the tooth, and is generally worn away from the crown; the central yellowish substance of the tooth presents a number of coarse canals, about one-tenth of a line in diameter; these radiate in a beautiful manner from the upper part of the pulp cavity, those in the middle proceeding parallel to the axis of the tooth, those at the circumference curving outwards. These canals are unequal, presenting partial dilatations, which, however, are sometimes, though rarely, discernible in the tubuli of human teeth; they give off numerous tortuous branches of different sizes, and these open into very distinct calcigerous cells scattered about the interspaces of the coarser canals; the fine crust of ivory above mentioned is formed by minute tubes directly continued from the finer ramifications of the large canals, and terminated in plexus of still finer tubes, which at length escape the highest magnifying powers. The fang, or inserted part of the tooth of the sloth, is coated with a layer of *crusta petrosa*, which is characterized by large canals and abundant *Purkinjian corpuscles*. There is no enamel in the composition of these teeth or of those of any of the existing Edentata.

Megatherium.—Microscopic examinations of the the structure of the tooth of this extinct mammifer have undeceived me with respect to its conformation, the thin dense layer between the crusta petrosa and the internal substance composing the body of the tooth, is not enamel, but a layer of ivory composed like the ivory of the highly-organized teeth of other mammalia, of minute tubes having a parallel course at right angles to the surface, and minutely undulating in that course. The body of the tooth consists of a coarser ivory, much resembling the teeth of *Psammodus* or *Myliobatis*, among fishes. It is traversed by large canals parallel to each other and to the finer ivory tubes, having angular interspaces equal to one and a half diameter of their own area, and generally anastomosing in pairs by a loop where convexity is close to the origin of the fine ivory tubes, as if each pair so joined was composed of one reflected canal. Some, however, are continued across the fine ivory, and appear to anastomose with the corresponding canals of the *cæmentum*, the interspaces of the coarse ivory tubes appear at first view granular, but they are principally occupied by reticular branches given off from the canals; some of these anastomosing branches are seen coming off from the concavity of the loops, and retrograding. Numerous minute cells are scattered about the terminal loops of the coarser ivory canals; the origin of the fine ivory tubes seem in many places to communicate with these cells. The fine ivory tubes are separated by interspaces equal to one and a half their own diameter; they divide and subdivide, growing smaller and more wavy towards the periphery or cæmentum; here their terminal branches assume a bent direction, and form anastomoses, dilate into small cells, and many are clearly seen to become continuous with the radiating fibres of the corpuscles of the contiguous *cæmentum*. The cement is traversed by large canals running like the canals of the coarse ivory parallel to each other and to the course of the fine ivory tubes, with interspaces of about five times their own diameter, occasionally, but rarely, dividing dichotomously, in which case the branches usually anastomose and form loops with the convexities towards and close to the outer layer of fine calcigerous cells, in which the fine ivory tubes terminate. The cement differs from the coarse ivory in the fewer number of canals, and more especially by the presence of the bone corpuscles or radiated cells in the interspaces. The irregular tortuous fine tubes forming a network in the interspaces, and especially those proceeding from the convexities of the loops, are much more clear than the corresponding tubes in the coarse ivory. The primary branches of the canals go off generally at right angles. This tooth exhibits perhaps the greatest amount of deviation from the ordinary mammiferous structure, and the nearest resemblance to the teeth of the lower vertebrata.

Of the teeth of reptiles, Prof. Owen described those of several genera, recent and fossil. In the *Sharp-nosed Alligator* (*Crocodilus acutus*), the exposed part of the tooth is covered with true enamel, and that part which is lodged in the socket is coated with a layer of *cæmentum*. The tubuli are very fine, not exceeding at the widest part $\frac{1}{1000}$ of a line. With a low magnifying power they appear to radiate in straight lines from the *cavitas pulpi* to the superficies of the tooth, proceeding at right angles to that surface: under a higher power, they are seen to be slightly undulating, and to have interspaces equal to five times their own diameters. The main tubes begin to divide soon after their origin, and the branches diverge from each other; these send off numerous finer ramuli, which are generally turned towards the root: these terminate or dilate, in many places, into calcigerous cells, which form numerous layers, generally arranged parallel with the contour of the cavity of the pulp, and most numerous at the circumference of the ivory. It is to these layers of calcigerous cells, and to the parallel curvatures of the tubes, that the apparent laminated structure is seen, by a low magnifying power, in sections of these teeth. A thin membrane lines the cavity of the pulp of even the oldest teeth. The fossil teeth of the extinct reptiles reveal an equally complicated structure. The fang of the fluted teeth of the *Ichthyosaurus* is covered with a thick layer of cæmentum, which fills the interstices of the grooves. The tubuli of the ivory-constituent are extremely minute; they resemble in their arrangement and ramification those of the crocodile, but the undulations are more numerous and more marked. In the *Iguanodon*, the ivory is composed of close-set tubes, radiating in a wavy course from the *cavitas pulpi* to the superficies: each tube is also minutely undulating. They are coarser than those of the Ichthyosaurus; and the ivory further differs in the presence of large (vascular?) canals which are seen here and there radiating from the cavity of the pulp.

In the teeth of the genus *Lamna*, a number of coarse canals are continued from the short and small pulp-cavity at the base of the tooth, which ramify and anastomose, so as to form a beautiful reticulate arrangement of tubes, very similar to a network of vessels, throughout the whole substance of the tooth: they ultimately terminate in a flattened sinus, which seems to extend over the whole tooth at a very short distance from its superficies; this space is occupied by minute calcigerous tubes, which proceed in a wavy course, generally at right angles, to the external surface; they ramify, and their terminal branches anastomose, and many of them terminate in a stratum of calcigerous cells, situated between the body of the tooth and what appears to be the outer stratum of enamel. In this stratum, however, there are evident traces of a series of much finer tubes, continued from the preceding layer of cells, which proves that this is not true enamel, but a fine kind of ivory, like that in the tooth of the sloth and megatherium. The coarse reticulate canals in the body of the tooth are surrounded by concentric layers; traversed by the calcigerous tubes, which are every where given off from the larger canals; these canals are occupied, in the recent fish, by a sanguineous medulla, closely resembling that which fills the medullary cells of the coarse bone, to which the base of the tooth is anchylosed, and with which cells the anastomosing reticulate canals of the tooth are directly continuous.

Carcharias Megalodon.—The calcigerous tubes at the superficies of this tooth are disposed in groups, which, with an insufficient magnifying power, appear like single coarse tubes, but with a higher power, are seen to be composed of congeries of

* The primary curvatures Prof. Owen explained to be those which belong to the general course of the dental tube, and which are seen with a lower power; in man they resemble the curves of the Greek zeta. The secondary curves are the minute undulations in the whole course of the tube, requiring a high power for their perception, and affecting both the main trunks and their branches; these indicate the movements of the formative pulp during the developement of the tooth.

parallel tubes, apparently twisted together. The interspaces are nearly equal to the diameter of these curious fasciculi: they are occupied by more scattered tubes, and by short oblique or transverse anastomosing branches. At one part of a section of this tooth, the peripheral coarse sinus or canal, which always runs parallel with the superficies, gave off an infinite number of minute tubes, which formed a plexus, (or plexiform stratum,) and from the outer part of this plexus, the tubes above described passed, at right angles to the surface. In the longitudinal section of this tooth, the twisted appearance, above described of the peripheral calcigerous tubes, was seen to be due to the number of side branches given off at an acute angle to the main tube. At the apex the tubes radiate, and suddenly diverge to proceed transversely to the sides. In the body of the tooth the main canals are surrounded by concentric lamellæ, traversed by radiating and anastomosing calcigerous tubes, which form a fine network in the interspaces.

The round palatal teeth of the extinct genus *Sphærodus* are anchylosed to a bone of a cellular structure. The body of the tooth consists of coarse tubes, which arise insensibly from the basis, and proceed directly and perpendicularly to the surface of the tooth. The characteristics of these tubes are, first, that they are so closely arranged together, that only one-fourth of their own diameter intervenes between them at their origins. Secondly, they begin immediately to give off short and somewhat coarse branches at very acute angles; these branches increase in number, and the trunks proportionally diminish, until they have traversed two-thirds of the vertical diameter of the tooth; they resolve themselves into fasciculi of extremely minute twigs, which interlace together, and in many places dilate into, or communicate with numerous minute calcigerous cells, and form so dense a layer as to intercept the light, excepting towards the circumference of the tooth, and consequently at the two extremities of the section, where only the structure above described is visible. Several small twigs pass beyond this plexus into the clear enamel-like outer layer of the tooth; in some parts of which traces are perceptible of a plexus of still more minute tubes, escaping the highest magnifying power employed in this examination.

Acrodus nobilis.—The crushing teeth of this extinct genus are composed of two substances, viz. a thin external almost colourless layer, which represents the enamels, and an amber-coloured coarser ivory composing the body of the tooth, and continuous with and passing into the coarse cellular bony basis and support of the tooth. Microscopic sections of this tooth afford the most beautiful appearances, and, perhaps, the most instructive illustration of the relation of ivory to bone. It consists of groups of coarse and beautifully branched and irregularly wavy tubes imbedded in a clear matrix. The main tubes radiate in all directions, from numerous points or cells which are surrounded by concentric strata, and closely resemble the canals of Havers in true bone: but towards the periphery of the tooth, the ramified tubes are all directed, as in true ivory, at right angles to the superficies; and thus constitute a regular layer of calcigerous tubes, disposed so as to offer the greatest resistance to pressure. This layer is equal in thickness to about one-fifteenth part of the vertical diameter of the thickest part of the tooth. The finest or terminal branches of this peripheral layer of tubes, I have traced in various places into what at first sight appears to be the enamel. Under a magnifying power of 400 diameters however, this outermost layer is seen to be composed of extremely minute tubes, $\frac{1}{1000}$ line in diameter; they are branched like the coarser tubes of the body of the tooth; irregularly wavy in their course; having a general tendency to an arrangement at right angles to the superficies, but inextricably interwoven, and connected anastomotically together, so as to require a strong light to penetrate even the thinnest section, and render their structure and arrangement visible. The continuation of these finer superficial tubes, with the coarser tubes of the body of the tooth, is best observed by changing the focus, which brings the transiternal tubes at different depths in the section into view. In some parts of the section, a Haversian canal is displayed longitudinally; and the parallel lines of the surrounding concentric strata on each side are exhibited. The canal maintains a general uniform diameter, but slightly dilates where it divides or sends off a cross branch to communicate with the adjoining canals. These canals commence from the large cells of the bone of the base, and pass into the substance of the tooth towards its periphery; communicate by tranverse canals, but all ultimately terminate in bundles of the wavy ramified coarse tubes of the body of the tooth. I conclude, that the longitudinal canals, as well as the coarse tubes, were occupied by a vascular pulp in the living animal—and that the fine terminal tubes were the seat of the salts of lime. The silex occupying the longitudinal canals and coarser tubes, has received a dark stain, probably from the colouring matter of the vascular pulp—but the finer tubes, from the want of this difference of colour, are in many parts obscurely visible, if at all. They are discernible in some situations crossing the concentric lamellæ at right angles to the central canal. The chief difference between the appearance presented by the Haversian canals of the tooth of Acrodus, and those in true bone, is in the absence of the cells or corpuscles. These are seen only at the base of the tooth: irregular in size and form, very minute, and appearing like simple granules without radiating lines. The character of the main or coarser tubes of the ivory of the tooth of Acrodus, reposes on their undulating course—their rapid diminution and branching, and the moderately acute angles at which the branches are given off, except at the circumference of the tooth, where they run nearly parallel to each other. They closely resemble the branching of trees. The line of demarcation between the coarser and finer ivory, is formed by a series of small cells of a similar granular appearance to those at the base, in which many of the finer branches of the coarse ivory terminate, and from which the minute tubes of the enamel-like ivory commence. The superficies of the tooth is slightly punctated, but the depressions do not correspond with the mouths of tubes, but with the interspaces of whole groups of the coarser tubes.

Psammodus.—A transverse section of the tooth of this genus presents the appearance, under a moderate magnifying power, as if it were composed of close-set coarse tubes, the areæ of which were thus exposed. Such a section, viewed with a power of 400 diameters, shows that these tubes are surrounded by concentric lamellæ, exactly as the Haversian canals; and that these lamellæ, and the clear interspace, which is generally equal to the thickness of the lamellæ, are permeated by minute irregularly convex tubes, which anastomose in the clear interspace, and open into extremely minute cells, scattered in the same part. A longitudinal section of the same tooth shows the whole course of the canals; they run nearly perpendicularly to the convex superficies of the tooth, and, consequently, incline outwards at the sides of the section. They lie nearly parallel with each other, with interspaces equal to from 6 to 8 times their own diameter, and branch dichotomously once or twice in their course. Each canal is surrounded by concentric layers of a dark colour, encroaching upon one-third of the interspace, which thus presents two dark streaks and one intermediate light line: the whole of these interspaces is perforated by the irregular wavy, branched, anastomosing calcigerous tubes. The terminations of the canals near the periphery of the tooth are slightly dilated, and give off in every direction calcigerous tubes, corresponding to those in the interspace of the canals. The structure of the tooth of *Psammodus* differs from that of *Acrodus* in the greater number and more parallel course of the canals: their fewer branches, and want of anastomoses; and in the absence of a distinct external enamel-like layer of very fine tubes.

Ptychodus latissimus.—The structure of this tooth has a close affinity to that of *Psammodus*: it is composed of Haversian canals and calcigerous tubes proceeding therefrom. The base of the tooth is composed of close-set and irregular canals, and is very opaque: the canals emerge from this part half way to the grinding surface, to which they proceed perpendicularly. They differ from those of the *Psammodus* in being wider, more close-set, and more branched; the branches being given off at more open angles, and the terminal ones being larger in proportion to the trunks. The papillose surface of the tooth is composed of the terminations of the inextricably interwoven fine calcigerous tubes given off from the terminations of the canals. The interspaces of the canals are also occupied by the same minute anastomosing reticulate tube-work. Numerous minute calcigerous cells are also present in the interspaces. There is a clear substance coating the grinding surface of the tooth, in which neither tubes nor any definite structure could be detected, though, from analogy, such doubtless exist. The darker substance, forming the concentric lamellæ around the canals, occupies the same proportion of their interspace as in the *Psammodus*.

In the summary of this series of observations which Prof. Owen detailed, he observed, that in the human tooth, and similarly highly organized teeth, the analogy of ivory to bone, as to texture, was only seen in the existence and intercommunication of the minute calcigerous tubes and cells; but that there was no trace of medullary or Haversian canals, with their characteristic concentric laminæ, unless the entire tooth were regarded as analogous to a single enlarged Haversian canal; the cavity of the simple pulp representing the medullary cavity of the canal; while the tubes, with the appearance of laminæ occasioned by their undulations, were equivalent to the concentric lamellæ and the calcigerous tubes, which, in bone, traverse these lamellæ, and radiate from the Haversian canal. In the teeth of many of the lower animals, however, and especially that of the extinct *Acrodus*, amongst the cartilaginous fishes, the resemblance of the dental tissue to bone was extended to the existence of the characteristic Haversian canals in great numbers. The presence of these canals was explained by the progress of the developement of these bone-like teeth, as observed by Prof. Owen in recent cartilaginous fishes. The large pulp, at the commencement of the formation of the tooth, had exercised its ordinary function in the secretion of a close-set series of calcigerous tubes, having a general direction perpendicular to the surface of the tooth, and closely resembling true ivory. The pulp, then, instead of continuing to form similar tubular ivory, by adding to the extremities of the previously formed tubes, became subdivided, or broken up into numerous processes, to which those forming the three fangs of a human grinder are analogous; but each process here becomes the centre of an active formation of similar branched tubes, radiating in all directions from that centre, and anastomosing by their peripheral branches with those from contiguous centres, or communicating with interposed calcigerous cells: the cavities containing the above subdivisions of the pulp, like the Haversian canals containing the processes of medulla in true bone, have had their area diminished in like manner by the successive formation of a series of concentric lamellæ; traversed, as in true bone, by radiating and minutely ramified calcigerous tubes, communicating with each other and with the minute cells in the interspaces. The resemblance of the pulp canals of the teeth of *Acrodus* and of the medullary canals of bone, is further exemplified in the existence of lateral communications in teeth; and in function as well as structure they might be regarded as being identical. With reference to the application of the tubular structure of the teeth to the explanation of their pathology, nothing has hitherto been attempted. Prof. Owen observed, that it was a new and fertile field, which would doubtless be replete with interesting results, and might suggest some good practical improvements in dental surgery. Ordinary decay of the teeth commenced, in the majority of instances, immediately beneath the enamel, in the fine ramifications of the peripheral extremities of the tubes, and, proceeded in the direction of the main tubes, and, consequently, by the most direct route to the cavity of the pulp. The decayed substance, in some instances, retains the characteristic tubular structure, which is also observable in the animal basis of healthy teeth after the artificial removal of the earthy salts. The soft condition of the decayed portion of a tooth is well known to all dentists; it depends upon the removal of the earthy salts from the containing tubes and cells, in which process the decay of teeth essentially consists. The main object of the dentist seems, therefore, to be, to detect those appearances in the enamel which indicate the commencement of decay; to break away the enamel, whose natural adhesion

to the ivory will be found more or less dissolved at the decayed part; to remove the softened portion of the ivory, and fill up the cavity with gold or other substance. Experience proves, what theory cannot account for,—viz. that the progress of the decay is sometimes thus permanently arrested. The calcareous salts are in such cases, as it were, poured out from the extremities of the tubes divided in the operation, and a thin dense layer intervenes between the exposed surface of the ivory and the stopping.

In conclusion, Prof. Owen passed in general review over the structures which he had described in detail, and remarked, that through the endless diversity which the teeth of different animals presented, the general law of the tubular structure could be unequivocally traced; and that the general tendency of the modifications observable in descending from man to the lower classes of vertebrate animals, was a nearer and nearer approximation of the substance of the tooth to that of ordinary bone.

Numerous sections of the teeth described, prepared for microscopic examination, were exhibited to the Committee.

Dr. Reid then gave a brief notice of his researches 'On the quantity of air required for respiration.' He pointed out imperfections in previous experiments, particularly that consisting in the small number of individuals experimented on. A great difficulty existed in attempting accurate conclusions, from the diversity of constitutional temperaments, different states of humidity of the atmosphere, the state of insensible perspiration, and also from the admixture of small quantities of foreign gases; in one instance, the admixture of $\frac{1}{3000}$ part sulphuretted hydrogen, was enough to "knock up" a whole room, producing very serious effects. The degree of light was also an important element, ten per cent. of carbonic acid produces much oppression in the dark; but, if strong light be admitted, it becomes tolerable. Dr. Reid stated, that at St. Petersburgh, he was informed by Sir I. Wily, that the cases of disease on the dark side of an extensive barrack, were in the proportion of three to one, to those on the side exposed to strong light, and this uniformly so for many years. Dr. Reid explained the mode he had adopted to ventilate the House of Commons, which he illustrated by diagrams, and demonstrated by the exhibition of a glazed model of the House. The current of fresh air could be introduced either from below or from above, diffused uniformly, and not by violent draughts, but as it were insensibly, and was under the most exact control as to quantity. The air, when used for the purposes of respiration and combustion, was conveyed away in an opposite direction to that in which it had been introduced. In answer to a question, Dr. Reid said, that he had taken no account of the products of the combustion by which the heat and light were produced, as these products should be omitted in all calculations on the subject. They, if possible, should be carried off so as not to interfere with the immediate supply to each individual. For the purpose of raising the temperature, hot water was used in iron tubes, not raised above 150°. Dr. Reid also stated, in answer to other questions, that he had not made any particular observations on the modifying influence of different articles of clothing, but he believed they did modify considerably the question, those being preferable that were of a very porous nature, allowing an *insensible* application of the atmosphere to the cutaneous surface.

Dr. Inglis read a paper containing 'Remarks on the skull of Eugene Aram.'

Proofs of the identity of the skull were first adduced. The head was removed from the body, which, according to the sentence, was hung in chains at Knaresborough, by Dr. Hutchinson, of that town, whose widow married a gentleman still living, from whom it passed into the hands of the Rev. Mr. Dalton, from whom Dr. Inglis received it. Mr. Dalton, it appeared, sent it to Spurzheim, who mistook it for a female skull. The paper stated, that the developement of the mental faculties as indicated by the skull, agreed in a remarkable manner with the character of Aram as recorded. Strictures on the nature and amount of the evidence by which he was condemned, were brought forward; and these strictures were enforced by the probable character of Aram, as indicated by the skull. A long and desultory conversation ensued, in which the evidence of the identity of the skull, was denied by some members, and acknowledged by others; and the importance and truth of phrenology were alternately asserted, and denounced as chimerical and absurd.

Section F.—STATISTICS.—Wednesday.

Dr. W. C. Taylor read 'An account of the changes in the population of New Zealand,' communicated by Saxe Bannister, Esq., late Attorney General for New South Wales.

It was stated that British emigration during the first five years after the peace averaged 5,000 annually, but during the last seven years, the average had risen to 70,000, and a portion of this stream having been recently directed to New Zealand, had given interest and importance to the statistics of that country. The New Zealand group consists of the Northern and Southern islands, Stewart's island, and some smaller islands; the extent of these is 95,000 square miles, or sixty millions of acres. The population was classed under the following heads:—natives, white residents, white visitors, and mixed races. The number of natives has been variously stated, but the most probable amount was said to be 130,000, or five natives to every three square miles. The white residents amount to about two thousand. In the Northern island, whites most commonly visit the Bay of Islands, and so many as 1,000 British and American sailors have been seen there at one time. In 1836 the following ships visited the Bay:

					Total
British Whalers	64	.. Ships of War 2	Traders	27	93
American ditto	49	....	Ditto	5	54
French ditto	3	....			3
Tahitian		....	Ditto	1	1
					151

There was no estimate of the mixed race, but it was stated to be greatly on the increase: the total population, however, decreasing, from a variety of causes, but chiefly from the introduction of European diseases. Mr. Bannister described at length the laws and usages of New Zealand, especially those relating to the tenure of land, which was said to be the property of the tribe, the chief possessing only the *suzeraineté*. The New Zealanders were described as a noble race of men, capable of attaining a high degree of civilization; but Mr. Bannister said, there was no doubt of their being addicted to cannibalism. [Our own opinion on this subject was expressed a short time since, in the review of Mr. Polack's work on New Zealand.] They have learned to use many European implements, and to form plantations, farms &c.; one native, who has taken the English name of Bailey, is chief mate of a vessel of 300 tons; and would have been captain, but for the Navigation Laws. The amount of trade in New Zealand was estimated at 4,500,000*l.* annually! The Church Mission of twenty-five persons, after fifteen years, has only 100 communicants; the Wesleyans, with only six missionaries, in ten years, were said to have 700 communicants: the difference was ascribed to the fact of the Church Missionaries having become landed proprietors. Wars are not so frequent as they were previous to the introduction of gunpowder, which has rendered warfare more expensive, and the Missionaries have frequently exerted themselves to reconcile feuds between hostile tribes with considerable success. But, Mr. Bannister observed, that the number of Europeans, both residents and visitors, is now so great, that some means must be immediately devised for organizing an administration of justice there.

The discussion on this paper turned on the possibility of civilizing barbarous tribes.—Dr. Bowring said, that there was already a government agent in New Zealand, Mr. James Busby, an intelligent and philanthropic man. He had lately received a letter from Mr. Busby, who stated that civilization was making progress among the natives, many of whom were settling down to agricultural pursuits. Mr. Busby had effected great good in Australia, in a most unobstrusive manner, and owed his present appointment to no other influence than that of his high character. It was the opinion of this gentleman, that knowledge and civilization would gradually make their way in New Zealand. Dr. Bowring also stated that the Arabs on the frontiers of Egypt were beginning to lay aside their nomade habits. He had recently met with Bedouin Arabs, who had taken land, and were settling upon it as cultivators of the soil. On entering into conversation with these children of the desert, they informed him that they perceived the time for their abandonment of savage life had come, and it was their opinion that it would be more to their interest and comfort to live as farmers than as robbers.—Mr. Rawson said, that with respect to Mr. Busby, he had no power, beyond the moral weight of his character; and he had written home to state his conviction that it was desirable to invest him with power.—It was also observed, that the nearer approach of the Pacific Islanders to the Caucasian race might account for their being more easily civilized than the Indians of North America.

Mr. Rawson read a report 'On the Fires of London.' The total number of alarms of fire attended by the London Fire Engine Establishment, during the five years, from 1833 to the end of 1837, was 3,359, or 672 on the yearly average: of these, 343, or 68 per annum, were false alarms, and 540, or 108 per annum, were fires in chimneys. Thus, the number of alarms was thirteen per week, and of actual fires four in every three days. Some of the false alarms had arisen from atmospheric phenomena, such as the *Aurora Borealis*. Of the 2,476 fires, the premises were wholly consumed in 145 instances; seriously damaged in 632; slightly damaged in 1699. An analysis was given of the presumed causes of total destruction, and it was observed, that the number of fatal fires had greatly increased. The winter months do not exhibit so large a preponderance of fires as might be expected. December presents the largest average, but the next in order is May. On comparing the number of fires occurring on each day of the week, it appears that there is a slight excess on Friday, and a decided falling off on Saturday. In relation to hours, the number of fires is at the minimum, from five till nine o'clock in the morning, when it begins slightly to increase, until five in the afternoon; at which hour the rate of increase becomes considerable, and continues until ten or eleven o'clock, when the number is at the maximum; from which time it gradually declines until the dawn. The number of wilful fires in the five years was thirty-one, or six per annum, which is as one in sixty-four to the number of fires of which the causes were discovered.

At the conclusion, reference was made to the great excess of fires in the Southern counties of England over the Midland counties. This was attributed, by Sir Charles Lemon and Mr. Felkin, to the use of thatched roofs. It was also stated, that Newcastle, notwithstanding the vast consumption of coal in the town, is remarkably free from fires of dangerous magnitude: and it was suggested whether, as the greater number of fires occurred in London about eleven o'clock at night, the practice of raking out the fire at bed-time, which is not done at Newcastle where coals are cheap, might not have some connexion with these conflagrations.

'Statistical Notice of the Asylum for the Blind at Newcastle,' by the Rev. J. M'Alister.

This asylum was projected only in the last autumn, and there are at present six persons in the asylum, and arrangements are making for the admission of others. The exact number of the blind in this district has not been ascertained, but in the parish of All Saints, containing a population of about 9,000, there are twenty-one blind persons, but it is to be observed that in this district are the lodging houses of many of the wandering blind. The best alphabet for the blind, was said to be the modification of the existing Roman letters, devised by Mr. Alston; but that, though it is a very practicable thing to teach the blind *to read*, it is exceedingly difficult to teach them to think accurately. In every instance where a visible image is introduced, the meaning is more or less vitiated; and the integrity of the mind, by constantly receiving what it cannot understand, is endangered.

Mr. Alston gave an account of the progress of education for the blind, as evinced by the increased demand for his raised printing, from America, and various parts of the continent.

Section G.—MECHANICAL SCIENCE.—Wednesday.

'An Improved Method of Constructing Railways,' by J. Price.

This method consists in fixing rails on a continuous

stone base, a groove having been made in the stone to receive a flange or projection of the lower side of the rail. The stones and rails are to break joint with each other, and the chair by which the rails are to be secured is to be made fast to the rail by a bolt, not rivetted, but slipped in. The chair is to be sunk until the top is level with the top of the stone, and fastened to it by two small wooden pins. Any sinking of the road is to be obviated by driving wedges of wood underneath the stone until it is raised to the required height. The chairs are to be fixed at about 4 feet apart, and to weigh, if of malleable iron, 14 pounds, but if of cast iron, 20 pounds: the rail to weigh 50 pounds per yard.

'On the Construction of a Railway with Cast-Iron Sleepers, as a substitute for Stone Blocks, and with continuous Timber Bearing,' by T. Motley.

The cast-iron sleepers, which are wedge-shaped, and hollow, having all their sides inclined inwards towards the under side, are to be laid transversely, and the timber is to pass longitudinally through the centre, and to be secured by wedges of iron and wood. The sleepers are to be six inches apart, and the timber of such a thickness as to prevent any perceptible deflexion betwixt the rails. The road is to be ballasted up to the top of the sleeper, and the timber to stand out sufficiently, and to have any approved rail laid upon it.

Mr. Stephenson considered the plan too expensive. —Mr. Donkin observed that a certain portion of elasticity was beneficial.—Mr. Vignolles said it appeared to him to be Mr. Reynolds's plan cut in pieces.

'Machine for raising Water by an Hydraulic Belt,' by Mr. Hall.

In this machine, an endless double woollen band, passing over a roller at the surface of the earth, or at the level to which the water is to be raised, and under a roller at the lower level, or in the water, is driven with a velocity of not less than 1000 feet per minute. The water contained betwixt the two surfaces of the band is carried up on one side and discharged at the top roller by the pressure of the band on the roller, and by centrifugal force. This method has been in practice for some time in raising water from a well 140 feet deep in Portman Market, and produces an effect equal to 75 per cent. of the power expended, which is 15 per cent. above that of ordinary pumps. This method would be exceedingly convenient in deep shafts, as the only limit is the length of the band, and many different lifts may be provided.

Mr. Hawkins had seen a machine very similar, fifty years ago.—Mr. Donkin, without entering on the question of originality, stated that he had seen a machine of this description working with a beneficial effect of 75 per cent., the beneficial effect of ordinary pumps being about 60 per cent.

'On Cliff's Dry Gas Meter,' by Mr. Samuda.

This instrument consists of a pulse glass, that is, two thin glass globes united by a tube. These globes are partially filled with alcohol, and hermetically sealed when all the air is expelled from their interior. In this state, the application of a very slight degree of heat to one of the globes will cause the alcohol to rise into the other. The pulse glass is fixed on an axis, having a balance weight projected from it, and the axis works in bearings on the sides of a chamber through which the gas to be measured is made to pass the gasometer in two currents, one of which is heated and the other cold. The hot gas is made to enter opposite to, and to blow upon the top globe of the pulse glass, while the cold gas blows upon the other. The difference of temperature thus established between the globes causes the alcohol to rise into the upper one, and the glass turns over on its axis, thus varying its position, and bringing the full globe opposite to the hot stream of gas. This stream, with the assistance of the cold gas, which condenses the vapour in the top globe, repeats the operation, and the speed at which the globes oscillate will be precisely in proportion to the quantity of gas which has been blown upon them, provided a uniform difference of temperature is always maintained between the two streams of gas. The difference of temperature is established and rendered uniform by a small flame of gas, which heats a chamber through which the lower current of gas has to pass, and the arrangements for securing an equality in the difference of temperature are very ingenious. The instrument is first tested by making a given quantity of gas pass through it, and observing the number of oscillations of the pulse glass. This once established, the instrument registers the quantity passed with inconceivable accuracy.

Considerable discussion ensued, during which many objections were raised, to which Mr. Samuda replied.—Mr. Liddell observed, during the progress of the discussion, that a flame consuming one-fifth of a cubic foot of gas per hour would burn in a chamber, and not be liable to be extinguished by the opening and shutting of doors; and that if due precaution were used, a flame might be preserved with a consumption only of one-eighth or one-tenth of a foot per hour.

'On the construction of Geological Models,' by Thomas Sopwith.

Mr. Sopwith lays down a method by which the commonest workman can make geological models, showing not only the position and thickness of the strata in a vertical section, but the actual surfaces and imbedding of the strata lying in different planes; so that one tray of the model being taken from above the other, we may consider that we have the stratum in miniature, with every undulation and indentation upon it. There was exhibited a model of the Forest of Dean, constructed in the following manner. The plan of the district was divided by lines crossing each other at right angles, and at the distance of a mile from each other. A vertical section was then prepared corresponding with each of these lines. These sections were drawn upon thin pieces of wood in the ordinary manner of a vertical geological section, and the several pieces of wood were then united by being half-lapped together, forming a skeleton model of vertical sections. After being thus united, the several sections were taken separately, and cut into as many portions as were required to illustrate the successive layers of strata; the intersection of each of these portions having been first marked by a number at the several corners. After each section has been divided into its several parts, these respective portions are again united, and formed the exterior boundary of a square mile of rocks. The interior of this is filled with wood, and carved so as to coincide with the sections. Any intermediate portion can be fitted in with great exactness, first by a thin or skeleton section, and afterwards by wood, which any workman can carve with the most exact accuracy as quickly and as surely as any ordinary mechanical operation; and thus at once a connexion between the most complicated section and the art of a common workman is accomplished. The outline of the surface, and the general contour of the country, is obtained, partly by means of the skeleton sections, and partly by the use of a gauge, or graduated pencil, sliding in a frame, in the same manner as practised by sculptors in transferring the dimensions of a cast to a block of marble.

'Description of an improved Levelling Stave, for Subterraneous as well as Surface Levelling,' by Thos. Sopwith.

The method of reading the figures of the stave itself, instead of the sliding vane, as adopted by most experienced engineers and surveyors, is used in Mr. Sopwith's improved staves. The figures are engraved on copper-plate, on an enlarged scale, so as to contract in drying to their proper length, which is fixed by a very accurate gauge. The arrangement of the scale is that of feet, divided into hundreth parts, alternately black and white; and in the form of the figures, clearness and distinct vision at a distance are chiefly aimed at. Mr. Sopwith's improvement consists in the mechanical arrangement of the slides, which are held in any fixed position by means of a catch or spring. The stave for mining purposes has also an entirely new arrangement. It has a glass shield or cover to protect the face of it from wet and dirt; and is hinged, so as to work in any seam of from three to five feet; but the principle may be adopted for any greater or less extent.

'On a Suspension Bridge over the Avon, Tiverton,' by T. Motley.

The peculiar feature of this bridge, is, that each chain is attached to the roadway, and the suspending bars are carried up through each chain above it. The length of the bridge is 230 feet, the breadth 141, and the cost, including the towers and land abutments, under 2,400*l.* This bridge is superior to the common suspension bridge, in that it is more firm, and experiences much less friction, owing to the absence of vibration.

Models were exhibited and partially explained of a suspension bridge of wire, erected over the river Avon, near Bath, by Mr. Dredge. The bridge is upwards of 230 feet in length, the breadth of the roadway is fourteen feet, and the whole, including land abutments, &c., was completed for less than 2,400*l.*—A method of Pumping Water from Leaky Vessels at Sea, by Mr. Dalziell. The machine is worked with a piston, the motion of the vessel being given by the stream when the vessel is sailing, to paddle wheels on the sides.—An instrument for Measuring Timber, by J. Smith.—A peculiar Combination for the Wheel Work of a Crane, by W. Horner.—A peculiar form of Steam Engine Boiler, in use at the Glass Works at Gateshead, by J. Price. The principal advantages of this boiler were said to be, the impossibility of collapsing, the rapidity with which it generates steam, and the small consumption of fuel.

GENERAL OBSERVATIONS.

The very full report of the important business transacted in the Sections, leaves us but little space for an account of the feastings and festivities. As a mere record of proceedings, however, it may be well to note, that the attraction of

Wednesday Evening was the promenade in the green market, which had been very tastefully fitted up for the occasion. Not less than 3000 persons were present.

On Thursday, at two o'clock, there was, as usual, a

MEETING OF THE COMMITTEE.

Invitations to the Association were received from Birmingham, Manchester, Glasgow, Sheffield, York, Kingston-upon-Hull, and Worcester.—It was resolved, unanimously, that the next meeting should be held in Birmingham, during the month of August, the exact day to be fixed by the Council and the local Committee.—The following were then elected officers: The Rev. Vernon Harcourt, *President;* Marquis of Northampton, Earl of Dartmouth, Rev. Dr. Robinson, of Armagh, John Corrie, Esq., *Vice Presidents;* R. I. Murchison, Esq., Rev. G. Peacock, *General Secretaries;* Prof. Phillips, *Ass. Gen. Sec.;* Messrs. Barker, J. Hodson, F. Ossler, and Dr. Blackstone, *Local Secretaries;* John Taylor, Esq., *General Treasurer;* Messrs. J. L. Moylatt, J. Russell, *Local Treasurers.*

In proposing that the meeting be held in Birmingham, Mr. Babbage objected to the great expenditure in feasts and entertainments, and observed, that all the men of science, with whom he had conversed, regretted it, and would gladly see the larger portion of the money devoted to the promotion of knowledge, by experiment and inquiry. The great cost of these entertainments prevented small towns, equally anxious to do honour to the Association, from sending invitations. Subsequently, on proposing Mr. Vernon Harcourt as President, Mr. Babbage complained of the false position of science in the social scale of England. He had heard it whispered, that one in a high station had said, that he knew little of science, and cared less for it; and that another, in a still higher, had applied to the pursuits and rewards of science the term humbug. From such men he hoped the British Association would never select a President. He was, however, happy to say that the class was small, and not greatly respected by the aristocracy itself. Another, and a much larger class, among the aristocracy, consisted of those who from circumstances and position, were unable to devote their time to the cultivation of science, yet possessing extensive knowledge in other departments, were able to appreciate its value to the country, and respect its cultivators. Many of these, from their wealth, were enabled effectually to advance its interests. This was a large and increasing class, and from this portion he should ever be glad to choose the officers of the Association. There was, however, a third class, who were attached to science, who cultivated and were acquainted with it; and when he considered how much more difficult it was for persons surrounded by the temptations of wealth and rank to acquire

knowledge, than for men in humble stations, he was always willing to pay a higher respect to such acquirements than to those of men equally informed under circumstances of far less difficulty.—Sir J. Herschel, in seconding the motion, paid a high tribute of approbation to the merits of Mr. Vernon Harcourt.—Prof. Whewell, in proposing the Secretaries, protested against Mr. Babbage's assertion, that men of science were a degraded race in England.—Mr. Babbage now announced his intention of resigning his office of Trustee, and in doing so, made some observations, which having been misunderstood, he explained more fully at the general meeting of the Committee, on Saturday.—The Duke of Northumberland, as chairman, checked the discussion, and said that he could not assent to Mr. Babbage's assertion, that men of literature and science were in a degraded position in this country; he had always been taught to look up to them with respect.

The following noblemen and gentlemen were elected to the new Council:—The Duke of Northumberland, the Earl of Burlington, Mr. Baily, Mr. Greenough, Sir C. Lemon, Mr. Lyell, Col. Sykes, Mr. Lubbock, Capt. Washington, Major Sabine, Mr. R. Hutton, Dr. Arnott, Prof. Whewell, Prof. Graham, Rev. D. Buckland, Mr. Gray, Mr. Brown, Mr. Owen, Sir J. Rennie, Dr. Lardner, Dr. Jenyns, and Prof. Wheatstone, together with the officers of the Institution.

The Chairman, in adjourning the meeting, lamented the unfortunate differences which had arisen, and begged leave to remind members of a pithy maxim, more valuable than any words of his own, "Concordiâ res parvæ crescunt, discordiâ maximæ dilabuntur."

Thursday Evening — *General Meeting.* — The members met at the Central Exchange, when a report was made by the Presidents of the Sections, of the proceedings of the Sections during the week. Before the arrival of the Duke of Northumberland and the commencement of the general business, Prof. Peacock, of Cambridge, introduced the extraordinary calculating youth, Mangiamele, when several difficult questions were put to him, all of which he answered correctly.—The proceedings of the day concluded with a ball at the Assembly Rooms, about 700 persons present.

Friday was the day fixed on for opening the Durham Junction Railway. The arrangements and invitations were on the most liberal scale, and the Mechanical Section adjourned over till Saturday, to enable the members to join the procession. The object of most interest was the "Victoria Bridge"—the entire length of which is 270 yards, and its width, within the parapet walls, 21 feet. There is a double line of railway over the bridge, with a flagged causeway for foot passengers. According to the *Newcastle Journal*, the arch over the river Wear is 160 feet span; from the foundation of the pier to the spring of this arch is 72 feet; from the spring to the crown of the arch the distance is also 72 feet; and from the crown of the arch to the parapet wall, is 13 feet, making in all 157 feet. From this, to obtain the height for the ordinary water level, we must deduct the solid masonry buried beneath the waves, which makes the observable walling 130 feet. This is considerably higher than the celebrated Sunderland Bridge, and (as Mr. Ingham, the chairman at the banquet observed), taken as regards *height* and *span*, is the largest arch in Europe. True it is that the arch of the bridge over the river Dee, near the city of Chester, is wider, and the Spanish bridge at Alcantara, near Lisbon, is more lofty; but, taking into consideration the united difficulties of extent of span, and height from the water level, the "Victoria Bridge" must stand unrivalled.—The day concluded with a Soirée at the Assembly Rooms.

MEETING OF THE GENERAL COMMITTEE, SATURDAY.

Sir Charles Lemon, in the absence of the President and Vice Presidents, took the chair, and before the minutes were read, the name of Mr. Yates, which had been accidentally omitted, was added to the list of General Secretaries. When the minutes of the proceedings of Thursday were read, Mr. Babbage complained that no record had been made of his announced resignation, in which he was determined to persevere; but he would not inconvenience the meeting by calling on the members to supply the place of so responsible an officer as a Trustee, until the next meeting of the Association, at Birmingham. As considerable misapprehension prevailed as to what he had said respecting his intended resignation, he now begged leave to state, that circumstances, connected with the Council and Officers of the Association had occurred, which rendered it impossible for him to act cordially with one of his co-Trustees, and he had, therefore, thought it better for the interests of the Association that he should retire. He need not state that his friend, the Treasurer, was not the Trustee alluded to. He had intended on the Thursday simply to give notice of his resignation, in order that the General Committee might not be taken by surprise, but be prepared to appoint his successor at Birmingham.—Mr. Murchison, in reply, said the circumstances to which Mr. Babbage had alluded he had explained to the Council, and he believed to their satisfaction.—Prof. Bache, of Philadelphia, and Prof. Ehrenberg, of Berlin, were added to the list of corresponding members. The Treasurer's report was then read:—

	£	s.
Compositions of New Life Members	£ 403	0
Annual Subscribers	1365	0
Ditto, Members attending from a distance ..	555	0
	2323	0
Amount received for Books	87	13
	£2410	13

The following grants were then agreed to:

Section A.—Mathematics and Physics.

	£	s.
For the reduction of meteorological observations	£ 100	0
For the reduction of observations on the stars ..	200	0
For improving the nomenclature of the stars ..	50	0
For comparing the level of Bristol with that of the English Channel	100	0
For tide discussions at Bristol	100	0
For continuing reduction of stars for a new catalogue of stars, preparing under the direction of a Committee of the British Association	500	0
For similar reductions in the catalogue of Histoire Céleste	500	0
For the preparation of instruments for magnetical observation, to be claimed only on the refusal of her Majesty's government to undertake the expense	500	0
For continuing observations on waves	50	0
For the translation and speedy circulation of foreign scientific memoirs	100	0
For tabular meteorological observations	15	0
For repairing the anemometer at Plymouth ..	8	10
For meteorological observations at ditto	40	0
For hourly meteorological observations in various parts of Scotland, selected by Sir D. Brewster ..	100	0
Total amount of grants to Physical Section	£2263	10

Section B.—Chemistry.

	£
For continuing Mr. West's experiments on the atmosphere	£ 40
For observations on the effect of sea-water on cast and wrought iron	50
For the effects of hot water on organic bodies ..	10
For continuing the table of chemical constants ..	30
For conducting galvanic experiments near Newcastle	20
Total amount of grants to Chemical Section ..	£150

A grant of 200*l.* to enable the Section to bring to this country Alexis St. Martin, the American, whose stomach is so peculiarly circumstanced as to afford opportunities for observations in organic chemistry and vital physiology (See *Athenæum*, No. 338.) was refused. The Committee of the Medical Section joined in the recommendation.

Section C.—Geology.

	£
For researches in fossil ichthyology	£105
Ditto quantities of mud and silt in rivers ..	20
For a report on British fossil reptiles	200
Total amount to Geological Section ..	£325

Section D.—Zoology and Botany.

	£
For experiments on the preservation of animal and vegetable life	£6

Section E.—Anatomy and Medicine.

	£
For continued observations on the sounds of the heart	£ 50
For similar observations on the lungs and bronchiæ	25
For construction of medical acoustic instruments ..	25
Amount of grants to Medical Section ..	£100

Section F.—Statistics.

	£
For continuing statistics of English schools ..	£150
Ditto .. .. of working population ..	100
For statistics of collieries on the Tyne and Wear ..	50
Total amount of grant to Statistical Section ..	£300

Section G.—Mechanical Science.

	£
For ascertaining duty performed by Cornish engines	£ 50
Ditto .. speed of American steamers ..	50
Ditto .. duty of engines not in Cornwall ..	50
Ditto the best form of sailing vessels	200
[Mr. Webster moved, and Mr. Babbage seconded a resolution, that this vote should be increased to 300*l.*, but the proposal was negatived.]	
For experiments on the hot and cold blast on iron ..	100
For ascertaining railway constants	20
For inquiries into marine steam engines	17
For instruments to ascertain the duty of marine steam engines—to Dr. Lardner	50
Mr. Fairbairne	[illegible]
Mr. Russell	[illegible]
Total amount to Section of Mechanical Science	£ [illegible]
Total amount of grants	£3742 10

The principal recommendations not involving grants of money, were—

That Prof. Bache should be requested to report on the meteorology of the United States.—That Prof. Johnstone should report on the connexion of Geology and Chemistry.—That the Council should prepare a general report on the progress of Geology.—That J. E. Gray, Esq. should prepare a report on British molluscous animals and their shells.—— Selby, Esq. V.P., a similar report on British ornithology.—Dr. Forbes a report on the pulmoniferous mollusca of Great Britain.—And that Prof. Faraday, aided by a Committee, should report on the specific gravity of steam.

The recommendations involving applications to the government and other public bodies, were—

That the astronomical establishment at the Cape of Good Hope should be extended.—That an arc of the meridian should be measured in India, and the standard of English and Indian observations verified.—That observations should be made on the effect of refraction in the Himalaya mountains, and also in Bombay.—That magnetic observatories should be erected in India.—That, in continuation of the Ordnance survey, the mine and mineral wealth of each district should be in some way indicated on the map.—And that an office should be instituted for the preservation and collection of mining records.

The following researches were recommended:—

The best mode of systematizing meteorological observations.—The Fauna of Ireland, and the Salmonidæ of Scotland.—The natural history of the insects that attack pines.—The pulmonary diseases of animals.

When the recommendations had been adopted, the following regulations were proposed, and passed unanimously.

That members should be requested to prepare abstracts of their papers for insertion in the reports.—That Section C. should be entitled the Section of Geology and Physical Geography.—That the Sections be divided whenever the number and variety of subjects before them renders such a course expedient.—That the sectional rooms shall, for the future, open at 10 A.M., and that members be requested, on entering their names, to state the Section they intend to join.

Thanks were voted to various bodies and individuals who had afforded facilities for the inquiries conducted by the several Committees.—R. Hutton, Esq., M.P., G. R. Porter, Esq., and Col. Sykes, were chosen auditors.

Dr. Granville presented a requisition for the formation of an Agricultural Section, and gave notice that he would, at Birmingham, move the appointment of such a Section at the first meeting of the General Committee.

Monday, the 26th.—The members of the Association favourable to the cause of National Education, held their annual meeting in the rooms which, during the preceding week, had been occupied by the Statistical Section. The chair was taken by Mr. C. Bigge. Dr. W. C. Taylor explained the constitution, working, and results of the national system of education in Ireland, the nature of the difficulties it had to encounter, and its present prospects of success.—Mr. Simpson spoke at some length on the advantages of infant schools, and of the moral improvement which had resulted from their establishment in Edinburgh and other parts of Scotland.—Mr. Robert Owen briefly adverted to the benefits which would result from a judicious system of moral training.—Prof. Bache, of Philadelphia, described the principal national systems of education established on the Continent—and Mr. W. Cargill, the state of education in Newcastle and its vicinity.—Nothing new or very important was elicited, but it was the largest meeting which has yet been held, and was remarkable for the absence of all controversial discussion.

OUR WEEKLY GOSSIP.

Once again our "potent, grave, and reverend" friends of the Association monopolize all the talk. Fortunately, their meetings do not take place in the publishing season, and we have no doubt that, before

the month is out, we shall be enabled, by the help of one or two more double numbers, to dispose of all such arrears as may have accumulated during our attendance on the philosophers. In the meantime, however, a Paris friend insists on "reading a paper" on the Bayadères, and all the more urgently because he was compelled to keep silence last week:

Paris, Aug. 20.

You will perhaps have no objection to receive a word or two concerning our last arrived novelty—the Bayadères—whose performances at the Tuileries, before the Royal Family, on Saturday evening, is elaborately discussed this morning in the *Journal des Débats*, after that journal's most flowery fashion. These nymphs are five in number, and, with their attendants, come from Tirouvendi, a small town near Pondicherry. Tillè, the eldest,—a rich woman in her own country,—is thirty years of age; the other damsels, Souadiroun, Ranghoun, and Amany, are between fourteen and sixteen, the dancing party being made up by Veydon, a little girl only six years old. They are all wedded to their deity—*i. e.* to the Brahmin priesthood, being attendants on a pagodah. While dancing, they are accompanied by three male musicians, of an inferior caste, each of whom bears his part on an instrument of but one note; the band consisting of a tiny pair of cymbals, almost hidden in the hollow of the hand, a pipe, and a tamtam. The beauty of their dance depends in great measure upon the movement of the performer's hips, as in the Spanish Cachoucha—but nothing can be more monotonous: they appear to have no more idea of its being *lubrique* than the Shakers of America, when engaged in their uncouth worship;—both being acts of religion. But, nevertheless, their dancing and their costume, as first displayed to a select set of connoisseurs, underwent considerable modification and *veiling* before they were exhibited to royalty. On the former occasion, the breast and shoulders were closely covered with gold tissue, and immense petticoats perfectly concealing the shape were gathered round the hips, but all between these two masses of drapery lay bare. To present thus the torrid zone of the human form at court and upon the stage, was pronounced not *comme il faut*; when, therefore, they danced before Louis Philippe, the Bayadères were totally enveloped in scarfs. Nothing, as I have said, can be more monotonous than their dancing; the inimitable rolling of their eyes, too, and the wave-like play of the figure, must be completely lost to a large audience—in short, they are cabinet performers, to be examined through glasses, like cabinet pictures. Every one in Paris, however, will go to see them once, which will suffice to make their trip lucrative. But, after all, was it fair in M. Tardivel to kidnap these poor creatures, and bring them to Europe, where they must lose *caste*, and where their devotional *pirouettes* can only last as long as other nine days' wonders?

GLOUCESTER MUSICAL FESTIVAL, 1838.

THE Hundred and Fourteenth Meeting of the Choirs of GLOUCESTER, WORCESTER, and HEREFORD, for the benefit of the Widows and Orphans of Clergymen in the three Dioceses, will be held on Tuesday, Wednesday, Thursday, and Friday, the 11th, 12th, 13th, and 14th of September, under the patronage of Her Most Gracious Majesty THE QUEEN.

Stewards.

The Rt. Hon. Ld. Ellenborough.	The Ven. Archdea. Wetherell,
Henry Thos. Hope, Esq. M.P.	The Rev. Richard Musgrave,
Purnell Bransby Purnell, Esq.	The Rev. Samuel Lysons.

Principal Vocal Performers—Madame Giulietta Grisi and Madame Albertazzi; Signor Ivanoff and Signor Lablache; Mrs. Knyvett, Miss Birch, and Mrs. Alfred Shaw; Mr. Knyvett, Mr. Hobbs, Mr. Braham, Mr. A. Novello, and Mr. Phillips.—*Leaders*—Messrs. Cramer and Mori.—*Conductor*—Mr. Amott.

The Instrumental Band and Chorus will consist of upwards of THREE HUNDRED PERFORMERS, being One Hundred more Performers than have been employed on any previous occasion; and as those only of the first-rate talent have been engaged, it is presumed that the effect produced in this Cathedral, the finest for Music in the Kingdom, will fully equal that in the Abbey, at the celebrated Westminster Festival.

For the EVENING CONCERTS, in addition to the most eminent vocal talent engaged, the Orchestra will consist exclusively of Performers from the Philharmonic and Opera Bands, thereby ensuring perfect unity and precision in the performances.

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For the superior accommodation of the Visitors, the Secured Seats in the Western Gallery at the Cathedral, and all in the Concert Room, will be provided with backs; and, on this occasion, the Crown Court will be floored over, forming a noble Refreshment Room, contiguous to the Concert and Ball Room.

The Ballot for Secured Seats will take place at the Bell Hotel, on Wednesday, September 5th.

Note.—The whole of the amount of the Tickets applied for, must be paid before the ballot takes place.—After the ballot, Tickets for Secured and other Seats, to be had of Mr. Jew, Westgate-street, and the principal Booksellers.

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